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ARTICLE I.

THE ANNUAL DISCOURSE.

THE PRESENT POSITION AND VALUE OF THE EXPLORATORY OR OPERATIVE DIAGNOSIS.

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DELIVERED JUNE 14, 1911.

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NOTE.—At an Adjourned Meeting of The Massachusetts Medical Society, held Oct. 3, 1860, it was

Resolved, "That The Massachusetts Medical Society hereby declares that it does not consider itself as having endorsed or censured the opinions in former published Annual Discourses, nor will it hold itself responsible for any opinions or sentiments advanced in any future similar discourses."

Resolved, "That the Committee On Publications be directed to print a statement to that effect at the commencement of each Annual Discourse which may hereafter be published."

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THE PRESENT POSITION AND VALUE OF THE EXPLORATORY OR OPERATIVE DIAGNOSIS.

Mr. President and Fellows
of The Massachusetts Medical Society: —

What advantage has exact knowledge over the best non-operative diagnosis? The chief advantage is that the patient receives every chance that the nature of his lesion permits.

Under what conditions should the exploratory laparotomy be encouraged? Under conditions of failure to deduce a positive diagnosis in a reasonable time limit.

What element in the pathology has the greatest importance in the decision for or against exploration? It is the prognosis. Hence the advisability of the exploratory laparotomy is largely a matter of prognosis, and consequently the study and the teaching of prognosis is the most important theme in practical medicine and surgery.

Going farther, what are the most important elements of prognosis? There are two: the first is the pathology of the disease; the second, the experience of the observer.

My remarks to-day are but an elaboration of these few themes, as full as my time permits. Experience during the most active and interesting period of the last quarter-century of wonderful progress makes each topic prolific in ideas which seem to me of pre-eminent importance, especially to the internist and to the surgeon.

Before touching upon the exploratory or operative diagnosis, let me say a few words upon diagnosis in general.

While my object in this address is to advocate, under proper restrictions, an impregnable demonstration of the

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truth, yet I am fully aware of the disadvantages of a rule of universal or general or prevailing exploration, if adopted by the physician and the surgeon—by the men who of all others should possess the highest skill in diagnosis. To assume that accurate diagnosis is impossible except through operative exposure of the disease, is to assume in our profession, both medical and surgical, a lack of perception, of deduction, of experience, and of common sense, which the facts do not warrant.

On the other hand, to assume that any human being possesses the power accurately to recognize that which can be recognized only by some miraculous power or instinct, is to assume the impossible. That an experienced observer may make an occasional guess that seems little less than instinctive, we all admit. In trying to repeat such a triumph, we know how often he signally fails.

We all have felt, a few times in our lives, the elation which the operative demonstration of our skill has proved, but the more extraordinary that success has been—the more unexpected and unfounded, unless upon instinct—the more have we known that accuracy has been but guess-work.

But in plain every-day cases we know well that we as a profession have acquired great skill in the recognition of conditions that twenty-five years ago would not have been recognized. A typical appendicitis, for example, a stricture of the intestine, an ovarian tumor with twisted pedicle, an extra-uterine pregnancy, a fibroid tumor of the uterus, an ovarian cyst—all these conditions are now recognized with an accuracy that approaches the certainty of a mathematical demonstration.

Even an acute pancreatitis with hæmorrhage, or a pancreatic cyst, a gall-stone impacted in the hepatic or common duct, a stone in the kidney or in the ureter—even these, I say, usually permit a diagnosis so sure to be right that the surgeon feels, in the absence of contra-indications, fully

justified in advising operation. In regions external and accessible he feels even more strongly than in the abdomen that his diagnosis is so accurate that he may base upon it operations of the greatest magnitude and importance. The breast, for example, the mouth and throat, the rectum and vagina—all present conditions accessible to sight and to touch, so that an exploration by which a growth, *e. g.*, is exposed is unnecessary. But in these regions easily within reach of sight and touch, we know that errors in diagnosis are not only possible but frequent. So in the exploratory operations which expose abdominal tumors, we know only too well how often the surgeon is wrong—how the tumor of gastric ulcer is mistaken for cancer, the fibroid for cancer, the malignant stricture of the sigmoid for the chronic diverticulitis—and so on.

Now if it is so easy to mistake the disease when it is in our hands and fully exposed to view—whether in the breast, mouth, rectum, or vagina—and if after the most thorough exploration its nature is still uncertain, how futile it is to say that we can be absolutely sure that we are correctly interpreting histories, and the evidence of our senses in regions so deep as to be at times almost if not quite inaccessible.

Considerations like these bring us to the admission that, in spite of all knowledge dependent upon skill and upon experience, many a curable disease may be allowed to undermine health or destroy life itself, and that through an unjustifiable confidence in our skill in diagnosis and more especially in prognosis.

A familiar—far too familiar—example of the deadly results of overconfidence in a diagnosis of benign growths is seen in breast tumors. I have for many years taught the diagnosis of breast cancer, and have warned my students and readers against postponing operation, lest the disease prove malignant and get beyond operability. Has this emphasis availed me in always avoiding a stupendous and de-

plorable mistake myself? It has not. I have found in two or three young women tumors without the ordinary physical attributes of cancer; tumors which I either removed without taking also the whole breast, or which I left alone, but which soon showed signs of malignacy and were removed too late for permanent cure.

In tumors of the throat and tongue one can usually tell the probable nature of the growth. So it is in the uterus and the rectum. But the more experienced the observer the larger the number of cases in which he will recall a too late recognition of the real malignant nature of the disease.

In the diagnosis and prognosis of real perceptible lesions, the question of exploration will be an easy matter, for it will mean that one of two or more possibilities will be present—a question perhaps of cancer or ulcer of the stomach, of ovarian or fibroid tumor, of gall-stones or duodenal ulcer, and the like. The really difficult case is the one in which the symptoms are wholly subjective, and in which there is not present a single physical sign. Even more difficult is the case in which the surgeon will not only find nothing that his art can relieve, but even a condition to which his exploration will add serious complications, as when he explores for appendicitis and finds typhoid fever, or worse, pneumonia; when he explores for an extra-uterine pregnancy and finds a normal one; when he explores for acute intestinal obstruction and finds nothing.

EXPERIENCE.

My work is so filled with the proofs of human fallibility that I do not have to go far back to find illustrations. I will take a case that I saw on the day before this was written. Since September, 1910, the patient, a young man, had been complaining of abdominal pain and loss of weight. He had been under the occasional observation of a skilful practitioner of medicine and surgery. He was always "taking

physic and complaining of rising pains." One Tuesday his pain became paroxysmal and severe. In a day or two it was so distressing that an eminent medical opinion was sought. On Saturday exploration showed an intussusception, which was successfully operated upon. Three months later a tumor in the right abdomen suggested malignancy.

The history of the case during the winter, with the presence to-day of a large, irregular, hard tumor in the region of the ascending colon, without fever or leucocytosis, suggested intussusception as a result of a tumor of the intestine in the first instance, like two cases of intussusception that I had seen.

In this most recent case, whatever existed or whatever persists, there arises the question of diagnosis; for all winter the abdominal symptoms, the cause of which might have been easily and safely demonstrated, ended in a pathological and mechanical complication of the very first magnitude and operative danger. True, the lesion was most skilfully and successfully treated, as far as the emergency went; but there remains a condition inexplicable and sinister, which an early operation would have demonstrated and possibly obviated.

And what was the objection to exploration in this case? What is the objection to exploration in all cases like this? The answer is the same—the unjustified dread of a surgical operation—an operation chiefly dreaded because of previous failures under diagnoses made too late for possible success, so late indeed that the briefest and simplest exploration proved fatal.

Scattered through my private records of twenty-five years are disasters of all sorts in acute and chronic lesions. In all these occurrences, more or less distressing, the one prevailing cause lies in the loss of timely opportunity.

Now let us come forward and frankly admit the cause of surgical disaster. But first what do I mean by disaster?

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Death caused by operation and by nothing else; death following an operation which delay has made hazardous; death or disability caused by technical and avoidable errors, themselves the result of carelessness or of want of skill; death or disabilities incident to all operations *per se*, no matter how experienced and skilful the operator, and unavoidable through human fallibility or through the occasional and possible complications of disease; immediate failures owing to loss of timely opportunity, as in the postponement of operation in acute emergencies like appendicitis, perforated stomach, extra-uterine pregnancy, and tumors with twisted pedicle, infections of the mastoid, empyemata, knee-joint infections, and Cæsarian sections, pancreatic and gall-bladder infections, and the late results of gastric and duodenal ulcer, renal infections, even strangulated hernia or torsion of the testicle. But of all, by far the most frequent and inevitable are the recurrences of malignant disease made probable or unavoidable by non-recognition of the call for relief in early symptoms, and the resulting loss of timely opportunity.

Let us not forget that we are discussing the place of the exploratory diagnosis in medicine and surgery. I have mentioned briefly that the chief indication for exploration is the experience of the past twenty-five years, with its failure thus far to convince our profession that diagnosis has not been sufficiently accurate thus far to permit us to grasp the situation—to permit the early application of the surgical remedy which symptoms are calling for, but which they far too often are calling for in vain.

Let me admit right here—not only admit for surgery and medicine, but claim as their greatest and proudest accomplishment, the wonderful development of diagnosis through operative demonstration of cause and effect. What was difficult in 1886—the diagnosis of appendicitis—is now almost child's play, even with its occasional mysterious and

inexplicable manifestations. Extra-uterine pregnancy; tumors with twisted pedicles; hæmorrhagic pancreatitis; gall-stones and gastric diseases; deeply seated tumors, even in the heart, so to speak, of the brain itself—all these diagnoses are made with an accuracy that is marvellous.

And yet error in diagnosis, failure to recognize operative indications, make the adoption of operative demonstration, in my opinion, one of the most important questions of the present day.

Why is the exploratory diagnosis imperative, when I have just claimed for medicine this wonderful accuracy? It is imperative because the diagnoses, for accuracy, depend upon a development of the disease so marked that the characteristic symptoms are unmistakable—and, here is the real trouble, when unmistakable, they are often practically irremediable. Either some pathognomonic early symptom must be discovered or we must explore upon a suspicion which some common and early symptom creates.

Now what are the objections to exploration? They depend upon the locality to be explored. The chief area is the abdomen, and we know pretty well the dangers of simple exploratory laparotomy. They must, of course, be dissociated from the dangers of operation upon the pathological condition found. They are not great. Dr. Homans used to make it an essential part of his study of abdominal tumors to examine the patient under full anæsthesia. The experience of many years has shown to my satisfaction that the exploratory operation *per se* has dangers hardly greater than those of the ether examination. Considering the conditions sometimes found after bimanual examinations, I am not sure that an incision large enough for digital or manual examination, with careful and thorough manipulation, is not less dangerous than the forcible bimanual compression of the pelvic viscera, or the deep and forcible palpation, say, of the epigastrium or the flanks. After bimanual ex-

aminations I have sometimes found rupture of cysts, of abcesses, of the intestine, of veins, and even of arteries, for I always make a pelvic rectal or vaginal examination, or both, under full anæsthesia, before opening the abdomen, to discover contra-indications—if any exist—to operation.

The dangers are shown to be small when nothing is found—they are shown to be small by the hundreds of operations like that for chronic appendicitis, for simple ovarian tumor, for simple gall-stones, and other uncomplicated operations even of considerable magnitude.

The chief objection—in fact, the only objection to the exploratory diagnosis is its possible failure to show disease. But if the exploration fails to show disease, so much the better. In suspected cancer of the stomach, what greater boon can the patient hope for than the demonstration that the disease does not exist, and that there need be no fear either of the disease or of a serious operation? It is no small satisfaction to be able to say that the symptoms exciting fear of cancer need no treatment for cancer. The less there is found. the less the danger and the greater the rejoicing. The greater the justification, the greater our thankfulness that the exploration has been made, and that the patient has had the best chance.

In other parts of the body than the abdomen, the objection to exploratory operation may be great or trivial. In the brain or spinal cord my experience would be against the exploration without a positive if not an impregnable diagnosis, because of the unavoidable danger to precious and vulnerable tissues by the slightest manipulations. There is the same objection to explorations of the thoracic and cardiac areas. To be sure, we have vastly less experience in these regions. Moreover, diagnosis in the early stages of disease is much easier and more positive.

Finally, in the explorations of external pathology, beyond the demonstration of benignancy or malignancy, there is

little need of exploration. If any rule as to neoplasms is to be laid down, it should be in favor of destructive rather than of exploratory surgery.

Some days ago I was consulted by a woman of sixty who for a long time had been suffering from increasing spasmodic pain in the stomach. Examination of the epigastrium showed that the stomach was in a state of spasm. I went over the case very carefully and advised operation in the near future. No tumor whatever could be felt. No positive diagnosis was made, excepting the diagnosis of something mechanical exciting acute and oft-repeated gastric spasms.

The case seemed to me one for exploration, whatever the diagnosis. The condition was clearly demanding relief. To spend time in studying the case would have been as lacking in common sense as it would have been in the case of a boy who had capsized and was drowning.

Two days ago I operated on this patient and found a very unusual lesion. I have had one such case in my life before. In that case an eminent pathologist failed to name the disease in twenty guesses. It was cancer of the jejunum.

How are we really influenced by our experiences toward conservatism or toward radicalism?

There have been in the past few months several cases in which our prognosis has been so faulty that under similar circumstances we feel greater uncertainty than we did before, and whatever we have done, whether of radicalism or of conservatism, we have regretted our decision.

And it is experiences like these that really make the man a radical or a conservative in surgery. Few men can go through two or three unfortunate experiences in surgery—some not even one—without a permanent bias. I have always thought that Dr. Homans's courage and persistence in ovariotomies, after the loss of his first four patients, was what made him the great surgeon that he was. I admit myself

the awful discouragement that follows a fatal operation in a new field; but I am not sure that in the end it is not better for the surgeon and for his patients if the first few cases are difficult rather than easy.

As most surgeons must have observed, there have been during the past two months many strange infections from a source perhaps not fully determined as yet. I have had a few of them, and have felt their great responsibility to the full. And yet the question presented to me was whether to operate or not; whether the patient's chances were not, on the whole, better under medical than under surgical treatment. Three patients had abdominal symptoms. It was a question whether those symptoms were really caused by abdominal lesions or whether they were referred to the abdomen from elsewhere. In the first patient the symptoms progressed favorably for two days and then suddenly became fulminating. The father and mother of the patient declined operation, and the boy died. The effect of this experience upon me was most depressing. I thought that we had allowed the boy to slip through our fingers with a peritonitis clearly indicating operation if not begging, as it were, for relief. Then came the experiences of others in which operation accomplished nothing. Two patients were brought to my attention. In the case of one—a boy like the one just mentioned—abdominal symptoms were present, but in a form so mild that I could not advise operation, though I had resolved that, no matter how trivial the symptoms, I would surely explore the next case. The symptoms immediately subsided. A third patient had symptoms strongly suggestive of appendicitis. I operated immediately. The appendix looked normal enough, but according to Dr. Whitney it contained a small ulceration. There was free bloody fluid in the abdominal cavity. The temperature rose in twenty-four hours to 104°, where it stayed for forty-eight hours, and then gradually subsided.

In three other cases, similarly introduced by tonsillitis, I found serious complications: in one a gangrene of the whole lower extremity, in a second a phlegmon about the hip, in a third a subclavian phlegmon.

The phlegmon about the hip resulted in an extensive gangrene of the *fascia lata*, with infiltration of the surrounding muscles and muscular interspaces—a condition which required deep incisions. The patient to-day is in a desperate condition of streptococcal systemic infection, and the prognosis is grave.

We are now at the end of a quarter of a century—the first—in which modern surgery has made its great advance. I date my own active work in the abdomen from 1886, when I performed my first deliberate operation upon the stomach and upon the gall-bladder at the Massachusetts General Hospital. Although there had been for ten years more or less abdominal surgery, that surgery, as compared with that of the present day, was very limited. Operations were confined largely to ovarian tumors, and Dr. Homans did most of the work. From the pelvis and the ovarian tumor the surgeon naturally went to other regions and to other diseases; and when Fitz described and named appendicitis, the surgeon, following and working with him, quickly learned to apply the benefit of mechanical treatment to all sorts of abdominal diseases.

Now that there really remains (so it seems) no other disease to conquer, no other abdominal area to explore, what is the lesson which we have failed to learn after so many centuries of professional work?

We have explored surgically every portion of the body. We can remove successfully—and to me it seems most wonderfully—the pituitary body, after a study of its physiology and symptomatology that permits a pretty sure diagnosis. We have seen—by thousands of explorations in various parts of the body—the connection between cause and effect,

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between disease and its manifestations, until diagnosis has become almost a science of mathematical demonstration. We have learned through experience to apply, with swift anatomical precision, the principle of asepsis so successfully that a simple exploratory demonstration of existing conditions, whether of health or of disease, is practically without mortality. If in explorations there is danger of death or disability or other disadvantage, we know the danger and can anticipate it—can estimate it as a part of the equation of error.

What is, then, the lesson that we have failed to learn after so many centuries of professional endeavor?

We have failed to learn the lesson of human fallibility, and to appreciate the necessity for absolute demonstration. In a word, we have failed to use intelligently the exploratory operation under conditions which experience ought to have told us were otherwise inexplicable and filled with possibilities of failure and of disaster.

Although personally—and for the benefit of the patient—I wish to convince the men of large experience and strong conservatism of the truth of my views, I am well aware that my proposition is one that, backed up by such men may, if followed universally by every operator, well qualified or not, work harm; yet I am speaking the truth, and if my views are followed by those unqualified to carry them into actual practice, so much the worse, not for the views, but for the laws which permit any one, whether qualified or not, to practice surgery. But that is another question. My object is to try to demonstrate the best course, assuming that those who will follow that course are well prepared to do so.

As far as possible I like to use arguments as near mathematical demonstration as possible. And it seems to me that, balancing in certain cases the advantages of the exploratory operation with its disadvantages, the operation may be

proved with almost mathematical precision overwhelmingly superior.

Take one man's experience with breast tumors, as a familiar and easily demonstrated example. How does the harm of removing a breast tumor compare in, say, a hundred cases with the benefit? If I prove that the possible benefit to the patient is a hundred or but twofold, it seems to me that I have established the desirability of the exploratory operation.

I will first consider the reliability of diagnosis in breast tumors, when made by men of large experience. Judging by my own experience, which embraces more than 1500 cases, a diagnosis of benignancy so positive that error is practically eliminated is impossible. I, for one, cannot make it, and I do not believe that any one else can. Certainly no man could satisfy me by his opinion, unaided by exploration, that it would be safe to leave a breast tumor in a woman over thirty. And in the case of any one near and dear to me I should never permit such a tumor to be left.

Now for the validity of my argument I must claim for myself ordinary intelligence and skill in taking histories, in making examinations, and in drawing conclusions; and I think it is perfectly safe to say that I represent the average physician and surgeon. If I cannot make the diagnosis by means of this ordinary intelligence and skill, I must be guided by some method of procedure that will eliminate this possibility of error.

My audience must remember that while admitting the possibility of error in diagnosis and in technique, with resulting disaster, I do not admit any considerable ratio of error. Our results are superb as far as the nature of cancer permits. But there is a chance to better them, and that chance lies in *early diagnosis*. The earlier the breast tumor is recognized, and the more thorough the operation,

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the better the result. Even if, to bring about so desirable a thing, all breast tumors are explored, that exploration is practically without danger, *per se*, and has the advantage of certainty, which in benign cases banishes horrid fear—no mean accomplishment.

Now if I have established by the overwhelming arguments of experience the validity of my contention in breast tumors, how much more imperative is the exploration of cases in which the possible tumor is beyond the perception of the senses, as in most stomach, duodenal and gall-bladder cases!

To illustrate the possibilities of surgery in diseases of the stomach essentially hopeless, suppose it were possible to attack cancer of the stomach surgically as early as I have demonstrated the disease in the course of hysterectomies, gall-bladder, and other necessary operations, when there is but a wart at the pylorus, or a flattened thickening of the lesser curvature no larger than a ten-cent piece. Can any one doubt that resection of such an area would succeed immediately and permanently in curing a large number of patients who now have before them nothing but hopelessness and suffering? The present lamentable condition of gastric surgery is the result of our timidity in the use of the exploratory laparotomy. As common-sense men, what else can we do except to explore under suspicion, when we know that certainty of diagnosis in gastric cancer means a formidable and perhaps fatal operation, and, if the patient survives, an early recurrence? We *must* explore under the suspicion of epigastric disease if we do not under suspicion of disease in other organs. What we need in the diagnosis of surgical diseases of the soft parts is something as simple, effective, and safe as the x-ray in injuries and diseases of the bone, in calculi, and in foreign bodies. But that we cannot hope to have. Were it not for the greater dangers, the exploratory operation would give us a good

substitute for gaining information. It would be even more reliable, and I am not sure that the fuller and more trustworthy information does not pay for the additional risk.

In a nutshell, my chief argument—that based upon experience—is that treatment based upon a diagnosis that must necessarily be fallible, instead of the right treatment, may be the wrong treatment; instead of giving a hope, it may take away all hope. Diseases in all localities in the body may be subject to fallible conclusions and wrong treatment, or to no treatment whatever.

The chief question to be considered is that of end results—by what course of treatment are the end results, on the whole, the best? And by what course is the immediate danger the less, and the remote benefits the greater? Can there be any doubt what this answer will be?

I am aware of the inadvisability of encouraging the present tendency in all cases toward surgical treatment. But if surgical operations are performed that ought not to be performed there are still more that ought to be performed that are not performed.

The evidence of men of large experience, both medical and surgical, should be toward restraint, on the one hand, and encouragement on the other.

As affecting the question of exploratory diagnosis, the most important element is human fallibility. Human fallibility affects prognosis the more or the less as the methods of diagnosis approach mathematical accuracy, for we must admit, in surgical cases at least, that, given certain pathological facts, the prognosis is a matter of simple and impregnable deduction, almost as certain as is a process of subtraction or division in mathematics.

In diagnosis, as I have long ago remarked, the chief source of error lies in the demonstration of the facts. In illustrating this paper I mean to use chiefly the diseases of the epigastrium and right upper quadrant, for it is here that

diagnosis is most difficult, and it is here where the most good can be accomplished by surgical methods of treatment.

Nothing is easier than to tell the outcome of treatment in this area, for if we know the diagnosis, we necessarily know the prognosis, whatever the treatment may be. And lest we lose the chance, great or small, of curing the disease, where cure is possible, we must establish beyond question the diagnosis, and that at a period when operation—if operation be possible—may be undertaken early.

On the one hand, great danger, and, with error, no possibility of relief; on the other hand, slight danger and great possibilities of relief.

The question that I am discussing has been the most important one throughout the years of my surgical experience. We must not be restrained too long by the conservatism of the past, or stimulated to excessive zeal by our hope for the future. The one thing that must be fairly and squarely and honestly met is human fallibility. Is or is not diagnosis a fallible human attainment, no matter how great the skill and the experience employed? Can there be the least doubt as to our answer? And in what class of cases is certainty of diagnosis the most essential? In the case of the disease that kills unless surgical remedy is applied and applied early, can there be the least doubt about the correctness of the answer? In chronic cases in which time for study is abundant, what is the wise course to pursue for both physician and surgeon?

Is it not the thorough study of the patient, the use of all modern methods of investigation, the painstaking trial of medical treatment? Can there be any doubt about the wisdom of this conclusion; and, considering the possibilities of suspected disease, what more imperative action is there than that of an exploration that will solve every doubt and make clear the future course of treatment? Who can object to the positive demonstration of cancer or its absence, when sus-

pected at a period early enough to permit operative cure? It seems to me that no reasonable man can object. And if he does object, upon what grounds? Is it the danger of the exploration? If nothing is found, there is practically no danger, as every surgeon's statistics show. True, explorations that reveal advanced and hopeless cancer have an excessive mortality; but we are talking of the early suspicion and the timely opportunity that certainty provides.

Again, how much in pain, suffering, and death does it cost to make by exploration a positive diagnosis?

My questions and answers are founded chiefly upon conditions assumed to exist in diagnoses of the epigastrium and right upper quadrant, where remediable conditions are common. But if these conditions are common, their operative curability increases inversely with their age: the earlier they are recognized and remedied, the less the danger and the better the prognosis; the later the recognition, the greater the danger and the worse the prognosis. Can any one dispute this statement of cancer of the stomach, of ulcer with mechanical destruction of function (stricture of pylorus), of gall-stone disease, and of other rarer mechanical lesions?

Let me repeat my question in a little different form. Does the patient pay—is he asked to pay in pain, suffering, and danger—any exorbitant price for information that will demonstrate fully and settle forever all doubts as to diagnosis and the proper course to follow? And, could so grave a question in any other profession or occupation be settled at so slight a cost, would there be a moment's hesitation?

The skill of the modern surgeon in these questions of life and death, happiness and sorrow, provides a solution that should be eagerly grasped—grasped with a heart full of gratitude and appreciation that such a blessing at so slight a cost is possible.

On the other hand, a weapon so prolific of good when rightly employed must be used with great intelligence and

skill lest it do harm in careless hands. A weapon in the hands of those who know how to use it is safe; in the hands of those who do not know how to use it, may be dangerous. Let enthusiasm be tempered with doubt, and a full realization of what exploration means, and of the great skill and experience necessary to interpret what it reveals.

If we are to make any progress toward the cure of these diseases which, through the difficulties of early detection, are now well-nigh incurable, in what direction does that progress lie? Is there any remedy thus far found except the operative demonstration of the pathology long before the pathological process has invaded hopelessly anatomical areas essential to life?

I admit that I see at present no reasonable remedy — no remedy at all, in fact, but the exploratory operation. But, going to the root of the matter, the real problem for us is to make the exploratory diagnosis unnecessary. One of the chief objects of professional endeavor should be the early diagnosis. But all that we at present can do is to establish "probable cause" — to bring forward an indictment, as it were, against the stomach, the gall-bladder, the duodenum, or the pancreas, in which sufficient evidence is accumulated to show that there is justification for surgical action.

The community, the general practitioner, the internist, the surgeon must be alert to discover the first sign of organic disease. I strongly hope that through the multiplication of early demonstrations, especially in the course of operations commonly performed—such operations as those for uterine fibroid, ovarian tumor, gall-bladder lesions, appendicitis,—through such observations I strongly hope that we shall learn to detect and remedy these numerous mechanical conditions which at present are explored only to be abandoned as hopeless. But in our endeavor to strike straight and hard at the cause of impending danger, we must expect for

a long time to strike many ineffective blows—many blows wide of the mark.

Thus far, as surgery has progressed, we have approached a perfection of technique that permits extensive operative demonstration without danger, or with danger so slight that it is negligible. I see no way to get above the present level of diagnosis except, in the presence of a strong fear that some condition incompatible with health and life is attacking a vital organ, to explore and find out the truth as soon as that fear has become a strong suspicion.

Certainty is worth a little risk, especially when certainty means a possible demonstration of disease at a time when it is easily curable.

For these reasons I must admit my strong inclination, under the restrictions of common sense, to the operative demonstration and diagnosis of suspected surgical diseases.

I venture to propose the following conclusion:

Operative investigation of suspected disease remediable only by mechanical or surgical means, when used by those fitted to make it, deserves the indorsement of physicians as well as of surgeons. Before resorting to the exploratory operation, all reasonable means of diagnosis should be exhausted, except in cases of emergency from obscure causes in which the necessity for mechanical remedy is self-evident.

In tumors of uncertain nature, especially those associated with areas or organs subject to malignant disease, and especially at the cancer age, exploratory demonstration of the tumor, and, if necessary and possible, microscopic determination of its nature, should, in the absence of contra-indications, be the rule.

In the multiplication of indications for mechanical and operative treatment, surgery has become a highly specialized art and broad science, for the practice of which special preparation is essential. For the practice of surgery, the study of the morbid anatomy of surgical diseases in the

living as well as in the dead, and its relation to symptoms, especially those diseases producing mechanical symptoms, should be, with the study of normal anatomy, the surgeon's chief endeavor.

Finally, the exploratory diagnosis should be restricted in its application in those cases in which certainty of diagnosis is essential to relief, and in which, from lack of surgical remedy, error means disaster or death.

ARTICLE II.

THE SHATTUCK LECTURE.

OBSERVATIONS REGARDING THE RELATIVE FREQUENCY OF THE DIFFERENT DISEASES PREVALENT IN BOSTON AND ITS VICINITY.

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DELIVERED JUNE 13, 1911.

OBSERVATIONS REGARDING THE RELATIVE FREQUENCY OF THE DIFFERENT DISEASES PREVALENT IN BOSTON AND ITS VICINITY.

By the provisions of the bequest whereby the Shattuck Lecture was established, it is stated that the annual essay shall be upon "the climate of the Commonwealth [of Massachusetts], *on the diseases of its inhabitants*, and on such other subjects as the said Society, or its government may select."

I have chosen in the present lecture to abide rather strictly by the italicised statement of this provision, and to study the diseases of the inhabitants of this Commonwealth and more especially of Boston and its vicinity. It is a fact worthy of our note and serious consideration, that although we have extensive and, on the whole, fairly accurate figures regarding the most important causes of *death* in Boston and its vicinity, we know almost nothing regarding the prevalence of the different non-fatal diseases excepting the infectious diseases. Danger to public health makes it obligatory that these should be reported to the State and City authorities. Aside from these contagious diseases we have no figures, so far as I am aware, bearing upon the relative frequency of the different diseases prevalent in this city and its vicinity, nor upon the distribution of these diseases among the different racial groups of the Commonwealth.

In the absence of any system of general reporting of all diseases by all physicians to any central health authority, I can think of no way in which we can arrive at any approximate idea of *the morbidity of this city* except the one

which I have chosen in this study, namely, the examination of hospital records and hospital statistics.

I. THE HOSPITAL POPULATION.

I recognize, of course, that only a minority of those who are sick go to any one of the great hospitals of this city, either as out-patients or as ward patients. Most people are treated in their homes by private physicians. Nevertheless I think the size of the minority which seeks hospital treatment has never been realized by most of us. In an attempt to estimate the number of persons receiving treatment at one or another of the larger hospitals of Boston, I have combined the figures from the latest available annual reports of the following hospitals: the Boston City Hospital, the Massachusetts General Hospital, the Boston Dispensary, the Eye and Ear Infirmary, the Carney Hospital, the Homeopathic Hospital, the Children's Hospital, the Mt. Sinai Hospital, the New England Hospital, the Lying-in Hospital, the Boston Floating Hospital, and the Boston Consumptives' Hospital Department. This list, as will be seen at once, includes by no means all of the institutions receiving patients on a large scale. The Long Island Hospital, St. Elizabeth's, the Infants' Hospital and others are for various reasons omitted. Nevertheless despite these omissions, which probably balance the duplications due to "clinic trotting," the figures for 1909 add up to a total of 45,074 ward patients, and 181,640 out-patients, making a total of 226,714. If the estimated population of that year be taken as 639,921, it is apparent that we are dealing here with 35%, a very respectable minority of all the inhabitants, and presumably therefore with a very considerable minority of all the sick persons in greater Boston.

It seems then that we shall not go very far astray by taking account chiefly of hospital figures when we are attempting to arrive at estimates regarding the relative

frequency of the different diseases from which the inhabitants of this Commonwealth have suffered in recent years.

II. SPECIAL DISEASES AND THEIR FREQUENCY.

Among the hospitals above mentioned there are only a few which publish detailed and comprehensive reports of the diagnoses of individual cases. I have selected for the purposes of more special study the *Boston City Hospital*, the *Massachusetts General Hospital*, and the *Long Island Hospital*. I have combined the figures for the years *1903-1910 inclusive*, that is, for a period of eight years in each of the three hospitals above mentioned. This furnishes a total of 611,733 cases from which conclusions may be drawn.

The general plan of my study has included the following problems:

(1) What are the commonest of all diseases occurring in this vicinity?

(2) What is the relative frequency of the different diseases of each organ and of each type; for example, infections, constitutional diseases, etc.?

(3) What diseases seem to be increasing or decreasing within this period of eight years?

(4) What is the incidence of certain diseases among the different racial groups?

I shall take the latter problem first.

(*a*) STATISTICS REGARDING VENEREAL DISEASE.

Many wild guesses about the proportion of adult males afflicted with venereal disease have from time to time been published for campaign purposes, but so far as I know there is up to date no solid basis for estimate upon this matter. Thus Dr. Prince A. Morrow, in his book on "Social Diseases and Marriage," (page 25), states that "We do know that gonorrhœa is the most widespread and the most universal of all diseases in the adult male population, embrac-

ing 75% or more. The prevalence of syphilis, though not nearly so universal, is variously estimated at from 5 to 18%." No authority or source for these figures is given in the text, and how they were arrived at I am unable to conjecture. A fairly thorough search through the treatises on venereal disease and through Prinzing's fine treatise on "Medical Statistics" (Jena, 1906) seems to prove that there are on this topic no figures which deserve any respect. We do not know how frequent gonorrhœa and syphilis are.

Taking first Dr. Morrow's statement that gonorrhœa is the most widespread and universal of all diseases in the adult male population, I have to present the following facts:

At the three hospitals above mentioned, during the eight years from 1903 to 1910 inclusive, there were treated 35,852 cases of the different pyogenic infections, 18,413 cases of tuberculosis, 10,197 cases of diphtheria, 5,546 cases of pneumonia, and only *4,819 cases of gonorrhœa* and its well recognized complications. I am quite aware that a considerable proportion of the gonorrhœal patients do not come to a hospital for treatment. It is probable that a larger percentage of the cases of this disease are treated by private physicians, more or less reputable, than is the case with most other diseases occurring among the class of persons who frequent hospitals. Nevertheless I think no one can believe that the discrepancy shown in the figures which I have quoted above could possibly be accounted for by the supposition that most of the gonorrhœal cases have gone elsewhere for treatment. These certainly would not make up the difference between 35,852 cases of pyogenic infections or 18,413 cases of tuberculosis and 4,819 cases of gonorrhœa.

So much for the place and frequency of gonorrhœal infection among other infections. I have not the least hesitation in saying that gonorrhœal infection ranks very far

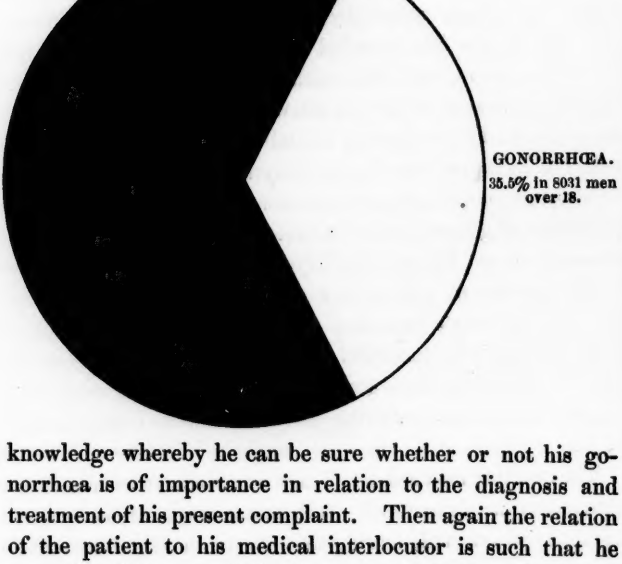
below pyogenic infection and tubercular infection in the number of persons attacked each year.

In the attempt to get a percentage which would be comparable with Dr. Morrow's 75% and would be based if possible upon verifiable data, I have studied the individual records of the Massachussetts General Hospital during approximately the same period as that already referred to, 1903 to 1910 inclusive. It has long been the custom at the Massachusetts General Hospital to ask of *every* ward patient, (not merely of those in whom physical evidence of present or past venereal infection was found), a set of questions which are intended to bring out whether the patient has had gonorrhœa and syphilis or not. These questions have been asked by many sets of house officers and the results are recorded not only with definite "yes" or "no" answers, but often with further detailed evidence. Omitting those in which the answers were defective or unsatisfactory, I have studied a total of 8031 cases who were asked definitely whether or not they had ever had gonorhœa or stricture or any other of the more easily recognized complications of gonorrhœal infection. These *8031 cases* were all *males over 18 years of age*. It did not seem practicable to arrive at any figures of approximate accuracy by questioning female patients; there must always be so many in this group who have had gonorrhœa without knowing it. But I believe that it is generally admitted to be very unusual for a man to have the disease without being aware of it.

The question next arises: "Are we to believe the patient's statements in this matter? Will he not frequently deny that he has been infected, fearing that the truth might in some way injure him?" In answer to these questions it is impossible to submit absolutely convincing evidence. I have questioned a large number of physicians who have taken the histories of many hundred ward patients, and whose judg-

ment and personal impression regarding the patients' truthfulness is, I think, of value. I find that it is generally the belief of the ex-house officers of the Mass. General Hospital, where these figures were collected, that the patients' answers are substantially correct. While it is true that the patient has motives for concealing the fact of his venereal infections, there are on the whole stronger motives for veracity. The patient fears that if he conceals the truth the physician may be misled in treating him, for the patient has no medical

TABLE A.

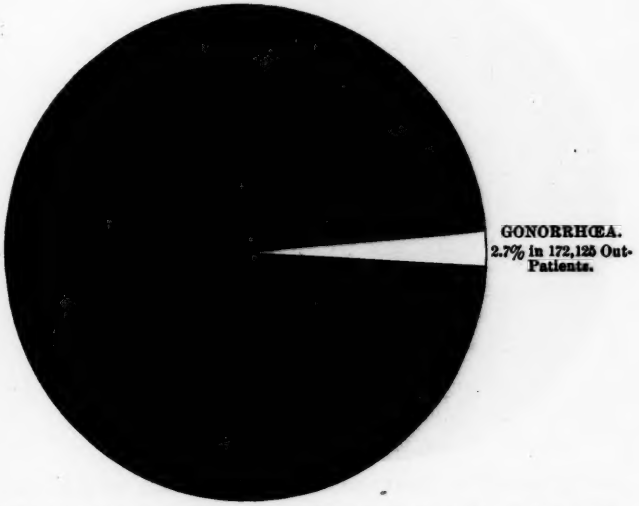


knowledge whereby he can be sure whether or not his gonorrhœa is of importance in relation to the diagnosis and treatment of his present complaint. Then again the relation of the patient to his medical interlocutor is such that he generally feels very little shame in confessing the truth, especially as it refers in most cases to what he regards as the remote past. For these reasons and for others which cannot be so definitely stated, I believe that the replies to these questions were substanially correct.

(*b*) FREQUENCY OF GONORRHŒA.

Now what is the result? Among 8,031 patients questioned, *35.5% admitted* that at some time in their lives they had had gonorrhœa (see Table A). This figure refers of course to all male patients over 18 admitted to the hospital wards for all sorts of diseases. None of these cases were admitted for gonorrhœa. This total of 35.5% is in marked contrast with the 75% given by Dr. Morrow. Dr. Morrow's figures refer, I take it, to all types and classes of persons.

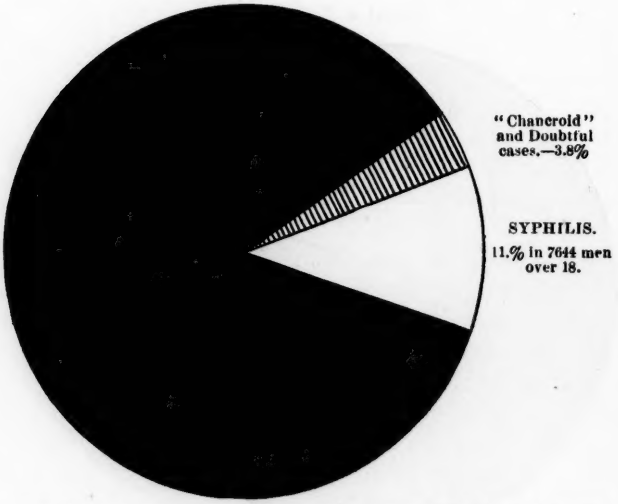
TABLE I.



My own figures, referring as they do exclusively to hospital patients (most of whom were treated free), correspond presumably to persons of the poorer, less educated classes. It is not likely that there is a larger proportion of gonorrhœa among *all classes* of society than among the *special class* which I have studied. Presumably if we could make a census of all classes of society, the figures would be smaller than mine.

In connection with these figures it may be worth while to notice the proportion of actual cases of gonorrhœa among the total number of cases treated in the Out-Patient Department of the Mass. General Hospital during the last eight years. Out of 172,125 cases of all diseases there treated (1903–1910), there were 4,886 cases of gonorrhœa and its complications, or 2.7 per cent. This figure of course is quite different from that previously discussed, and is in-

TABLE II.



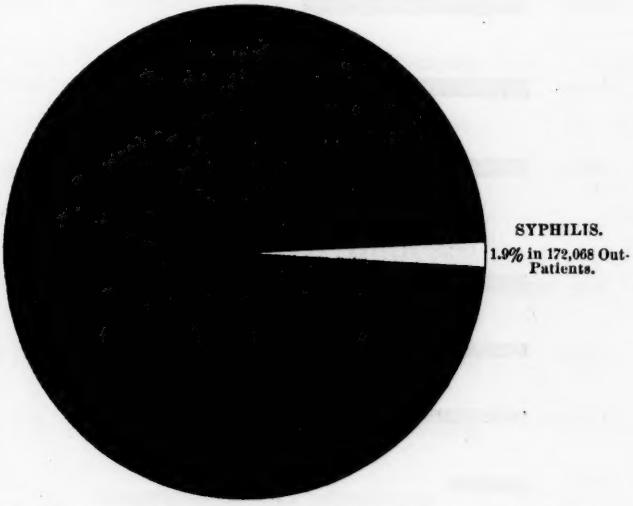
tended merely to convey some idea of the bulk of this disease compared with all diseases. These facts are graphically represented in Table I.

(*c*) FREQUENCY OF SYPHILIS.

Turning now to the question of syphilis, we find figures which more closely resemble those given by Dr. Morrow. I have included as syphilitics, all those who definitely stated that they had had syphilis, all those who stated that they had had a chancre, all those who stated that they had had

a chancre *plus* alopecia, or sore throat or an erythematous rash, a tabes, or a dementia paralytica following the primary lesion. All cases of *chancroid* and all cases where the only evidence is a scar upon the penis have been excluded from the positive class. Dr. Morrow says 5 to 18% of syphilis. Our own figures in 7,644 cases questioned show *11% of positive histories of syphilis* with an additional 3.8% of cases in which the history or physical examination suggests but does not positively affirm syphilis. (Table II.)

TABLE III.



Our figures then are either *11% or 14.8% of syphilis among the males over 18* questioned as a matter of routine in the wards of the Massachusetts General Hospital.

These figures refer of course for the most part to events which happened ten, twenty, or thirty years ago, and not to the present time. It would be interesting to make a similar research ten years hence and see whether the percentage has risen or fallen.

In the Out-Patient Department the syphilitic cases make up 1.9% of the total 172,068 cases coming to the hospital in 1903–1910. (Table III.)

TABLE IV.

NATIONALITIES: HOUSE SERVICE. 1903–1911.

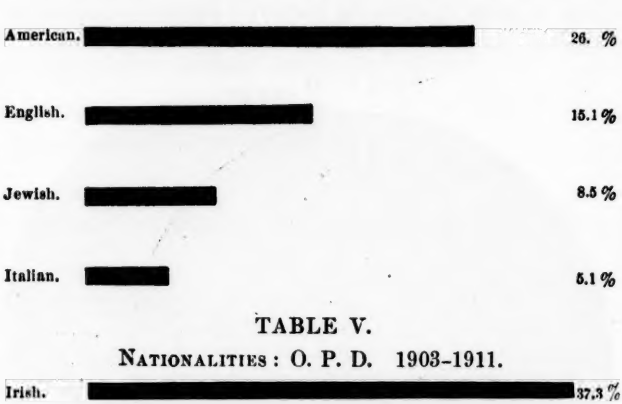
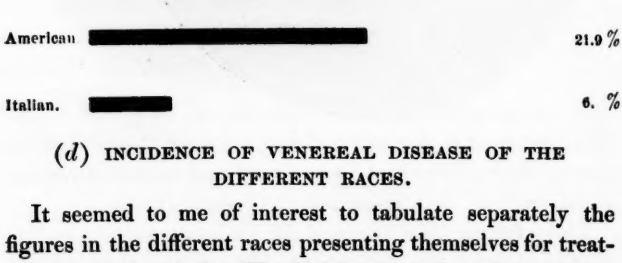


TABLE V.

NATIONALITIES: O. P. D. 1903–1911.



(*d*) INCIDENCE OF VENEREAL DISEASE OF THE DIFFERENT RACES.

It seemed to me of interest to tabulate separately the figures in the different races presenting themselves for treatment at the hospital. The divisions are of course not absolutely clear and cannot be made quite accurate. Nevertheless I believe the percentage of error is not large. Under the

head of "Americans" I have included those born in the United States and having names which do not suggest foreign origin. Under "English" I include those born in England, Scotland or Canada, *excluding* the French Canadians. Under Irish I have grouped those born in Ireland and those born in this country, but showing by their names

TABLE VI.

SYPHILIS: RACIAL DIFFERENCES.

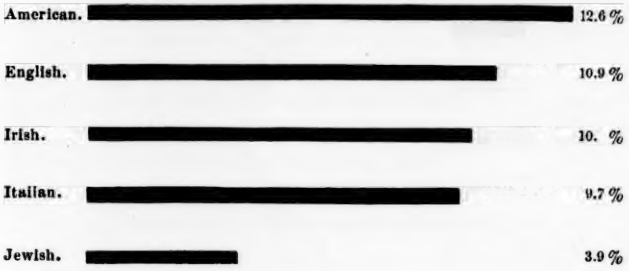
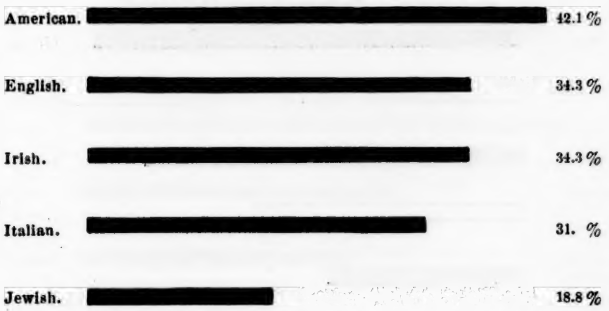


TABLE VII.

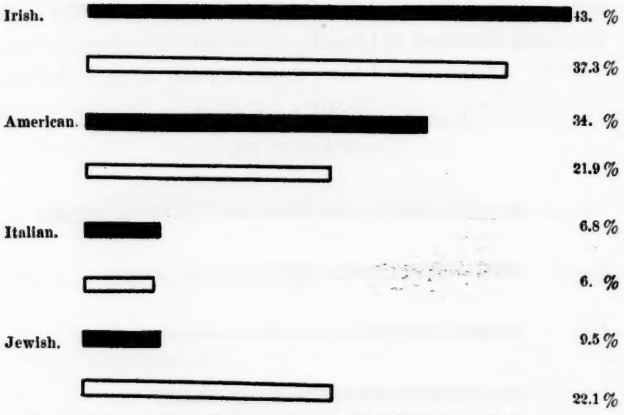
GONORRHŒA: RACIAL DIFFERENCES.



unmistakable evidence of Irish origin. Similar criteria have been used in separating out the Italian and Jewish groups. (Those born in Russia were counted as Jews.) These five races make up a large proportion of the total hospital population. Their relative proportions in ward patients and in out-patients respectively are shown in Tables IV and V.

TABLE VIII.

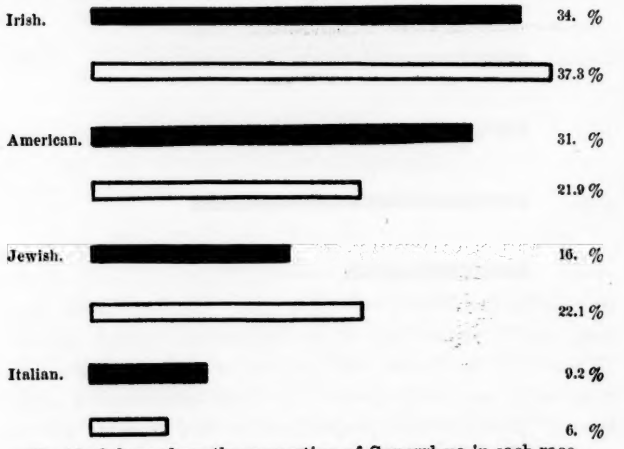
PER CENT. OF SYPHILIS IN OUT-PATIENTS OF FOUR RACES.



The black bars show the proportion of Syphilis in each race.
The light bars show the total proportion of each race coming to the Out-Patient Department for *all* diseases.

TABLE IX.

PER CENT. OF GONORRHŒAL DISEASES AMONG OUT-PATIENTS OF FOUR RACES.



The black bars show the proportion of Gonorrhœa in each race.
The light bars show the total proportion of each race coming to the Out-Patient Department for *all* diseases.

In the out-patient figures Americans and English are combined.

In collecting these data great pains were taken to avoid

TABLE X.

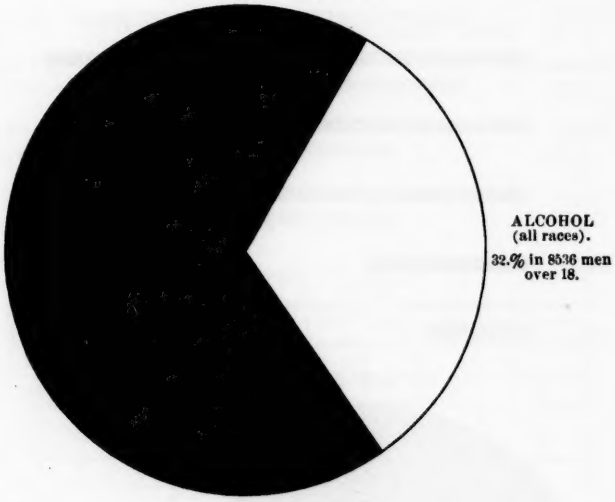
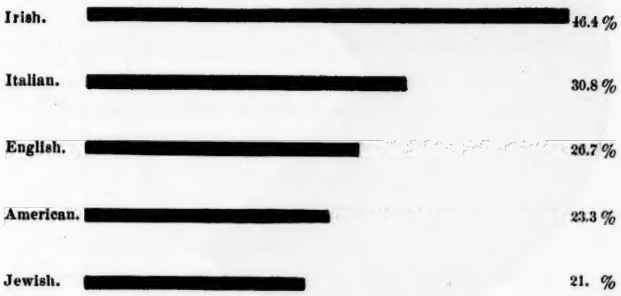


TABLE XI.

ALCOHOLISM: RACIAL DIFFERENCE.



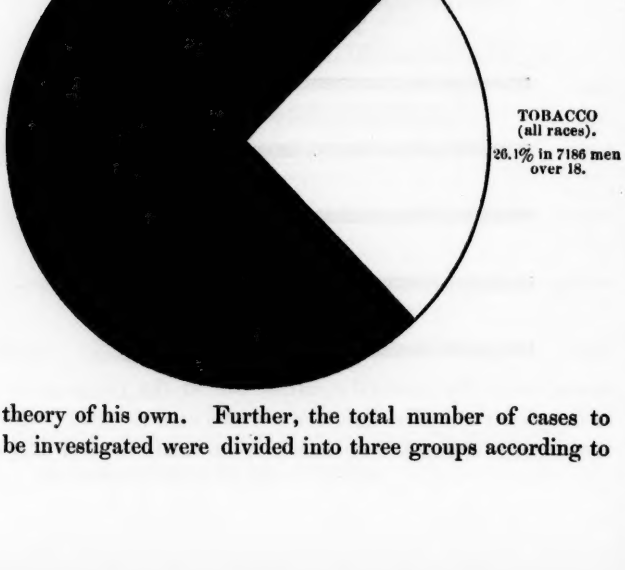
errors due to the personal equation and to the expectations of the observer. The figures were tabulated in such a way that it was impossible to form any estimate of how they

were coming out until the research was finished and the totals were added up. It was thus impossible for the observer to push or bend the figures in the direction of any

TABLE XII.

TOTAL ABSTAINERS: RACIAL DIFFERENCE.

TABLE XIII.



theory of his own. Further, the total number of cases to be investigated were divided into three groups according to

TABLE XIV.

EXCESS OF TOBACCO: RACIAL DIFFERENCE.

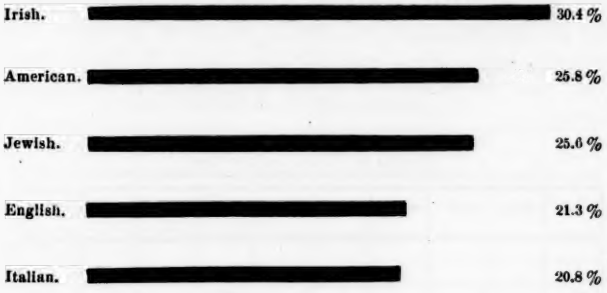
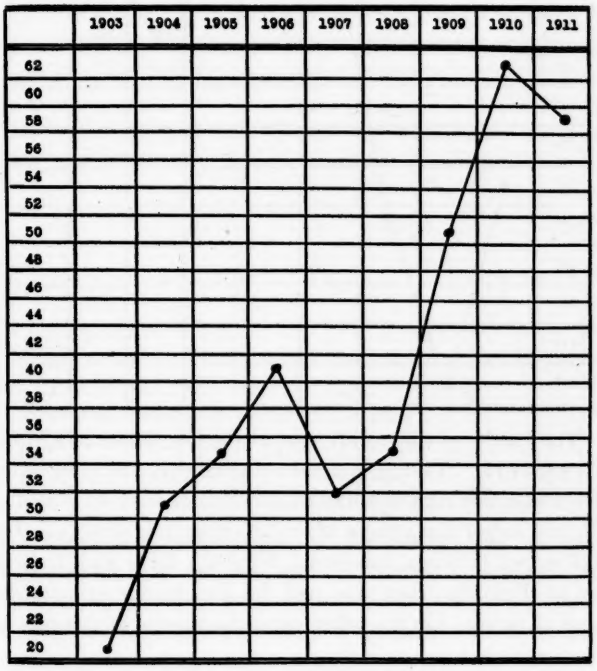
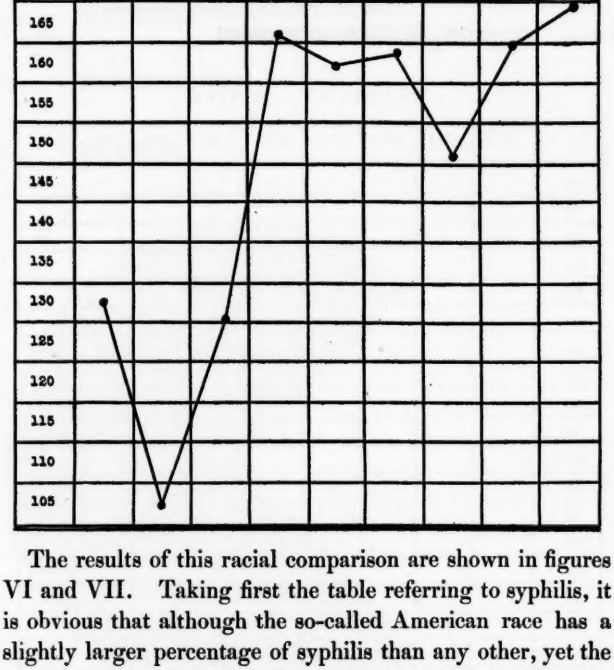


TABLE XV.—ITALIAN.



the different hospital services, and tabulated separately. The three sets of figures worked out separately were found to tally very closely. This tends to show that the personality of a given group of house officers on whose questions the data are based cuts no considerable figure in the results. Also that the tabulation of the enumerator is reasonably correct.

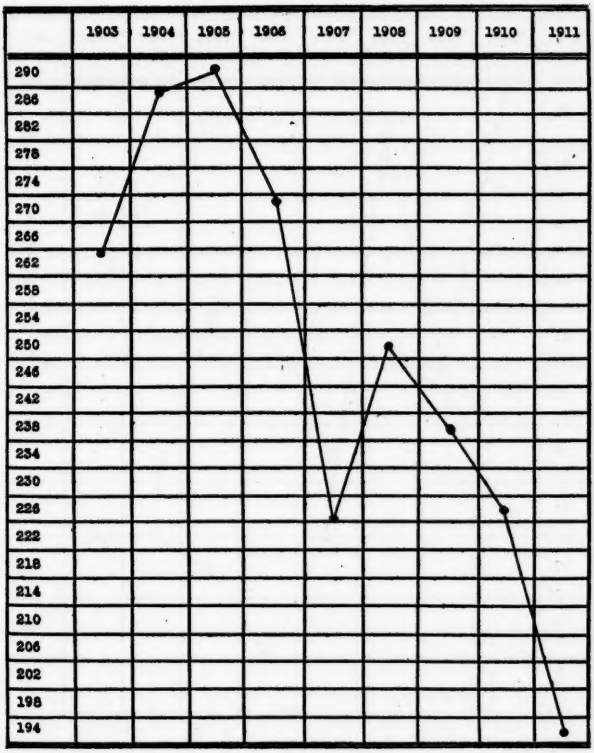
TABLE XVI.—JEWISH.



The results of this racial comparison are shown in figures VI and VII. Taking first the table referring to syphilis, it is obvious that although the so-called American race has a slightly larger percentage of syphilis than any other, yet the differences between American, English, Irish and Italian are so slight that they may well be due to one or another kind of error. On the other hand, the figures in this whole

group is in very striking contrast with the *much smaller figures corresponding to the Jewish patients.* The discrepancy is so marked that I have taken every method that I can think of to explain it away. Can it be due to the fact merely that the Jewish patients were less truthful in

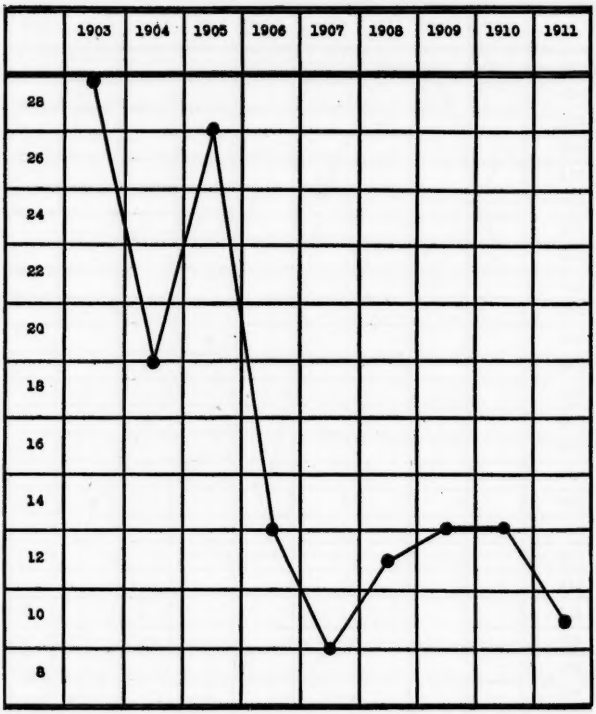
TABLE XVII.—IRISH.



their answers, or less accurate in their memories? It has been suggested to me that certain features of the Jewish religion might make these patients more unwilling than others to state the actual truth. But against this hypothe-

sis we have the evidence of the proportion of existing syphilitic disease among the patients coming to the Out-Patient Department for treatment. Here we are dealing not with possibly fallacious answers but with the actual facts of disease as found by the Out-Patient physicians and surgeons.

TABLE XVIII.—GERMAN.

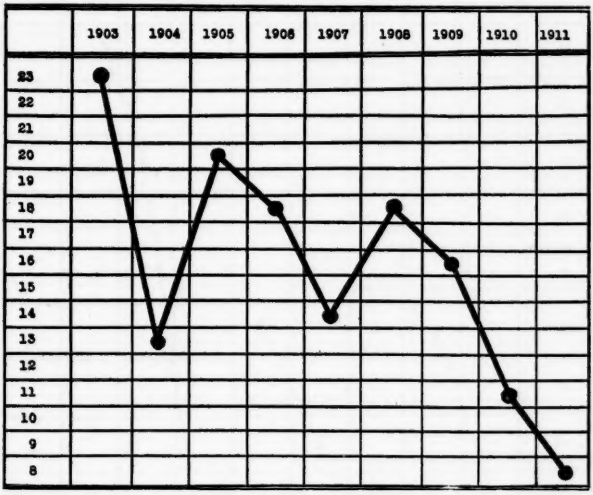


These figures show the same marked discrepancy between the number of syphilitic Jews and the number of syphilitics of other races. Here again the Jews have by far the best record.

In Table VIII, which is a graphic representation of the Out-Patient figures relating to syphilis, I put side by side

the proportion of syphilitics of each race and the proportion of that race among all the patients coming to the Out-Patient Department. From this table it will be evident that while the proportion of syphilitics in the Americans, Irish and Italians is at least equal to the proportion of each of those races coming to the Out-Patient for diseases of all kinds, the proportion of syphilitic Jews is much smaller than the total proportion of Jews coming to the hospital for other diseases. These figures, then, go very far to prove

TABLE XIX.—NEGRO.

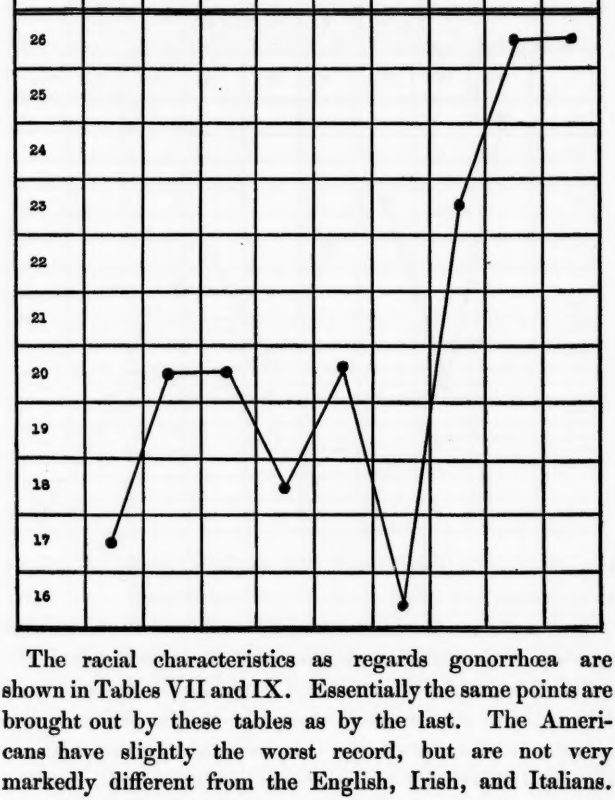


that there is actually much less syphilis among the Jews than among any other group of our population.

One may speculate regarding the influence of the Jewish custom of circumcision in this matter. Personally I see no good reason to believe that it plays any large part, but I know of no way in which this point can be definitely decided. It is my own belief that the figures correspond with an actual superiority of the Jewish race in sexual morality.

Doubtless this superiority is more marked in the older members of the race than in the rising generation. My figures refer of course far more to the past generation than to the boys now growing up, and from what I know of the rising generation of Hebrews I see reason to believe that its morals are far looser than those of the last.

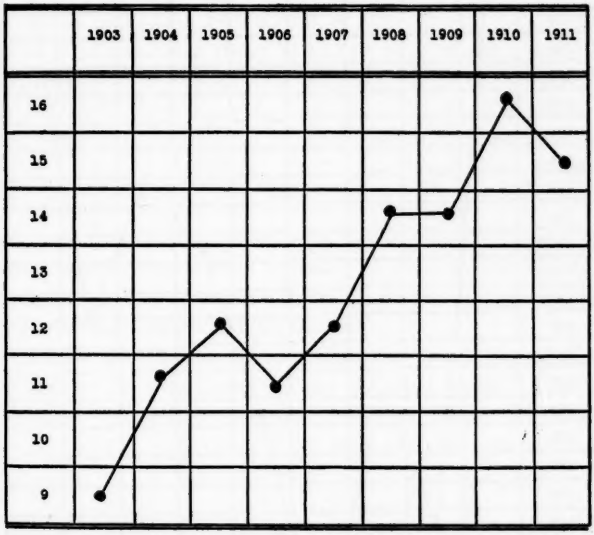
TABLE XX.—FRENCH AND FRENCH CANADIAN.



The racial characteristics as regards gonorrhœa are shown in Tables VII and IX. Essentially the same points are brought out by these tables as by the last. The Americans have slightly the worst record, but are not very markedly different from the English, Irish, and Italians.

On the other hand *the Jews have about half as much of this disease as the other races.* The discrepancy between the Jewish people and the other races is less marked as regards gonorrhœa than as regards syphilis; there is about half as much infection with gonorrhœa among the Jews, and about one-third as much infection with syphilis, as compared with the other racial groups in Boston.

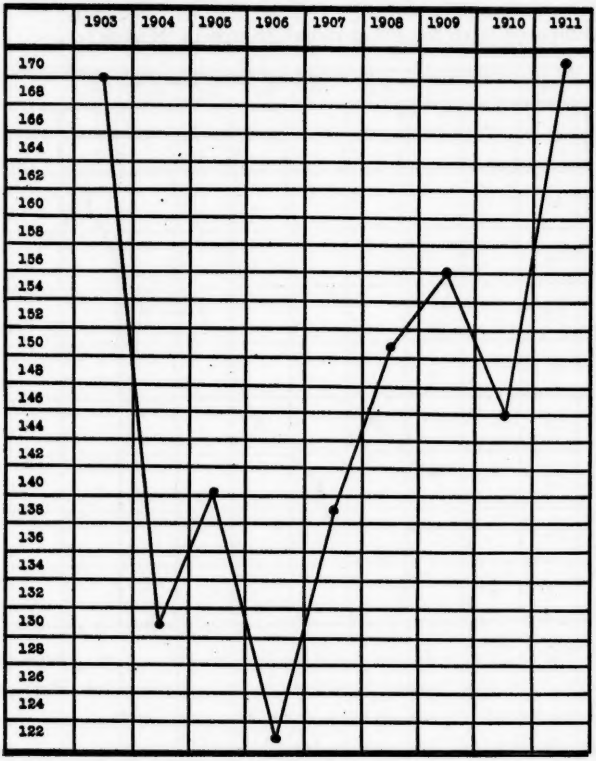
TABLE XXI.—SWEDE AND NORWEGIAN.



These figures do not surprise me at all. Five years of service in the female medical room of the Out Patient Department impressed very strongly upon me the comparative freedom of the married Jewish women from gonorrhœa. The number of cases with inflamed or adherent organs in the pelvis was very strikingly less among the Jewish women than among the women of any other race there examined. On the other hand, I am distinctly surprised to find that the percentage both of gonorrhœa and of syphilis

is so small among the Italians. It had been my impression previously that syphilis especially was more prevalent than among any other of the races that I had dealt with, but my figures do not bear out this idea at all.

TABLE XXII.—AMERICAN AND ENGLISH.



III. ALCOHOLISM.

Along with the data regarding gonorrhœa and syphilis, are there recorded at the Massachusetts General Hospital facts relative to alcoholism and the abuse of tobacco. Our

records are taken in such a way as to state not merely that there is or is not an excess of alcohol taken by the patient, but in detail how much is consumed and in what form.

For the purposes of this study I have assumed that all those may be called alcoholic or may be said to use alcohol in excess, who belong to any one of the following classes: (1) Those who admit that they frequently get drunk; (2) Those who admit that they frequently drink liquor before breakfast; (3) Those who take more than three whiskeys a day or more than half a pint of the liquor; (4) Those who take more than three bottles of beer or six glasses of beer daily; (5) Those who take more than two drinks of gin a day. It is open to any one, of course, to criticize my criteria for alcoholism, but no one need misunderstand them. Judging in this way, it appears that 32% of 8,356 patients of all races used alcohol in excess (see Table X). I believe this represents about the proportion of alcoholic excess (as above defined) in the total group from which hospital patients are drawn. In healthy persons the figures would be essentially the same.

I have been interested to tabulate the proportion of alcoholism (defined as above) in the different races. The results of this computation are shown in Table XI, which contains some rather surprising features. Particularly unexpected is the fact that the Americans so-called come pretty near to having the best record of all, being only slightly surpassed by the Jews! This is borne out by the further tabulation of the number of *total abstainers* shown on Table XII, wherein it appears that among Americans there are more total abstainers than in any other race.

I was also considerably surprised at the size of the figure corresponding to the Italian race. Those of us who are frequenters of the slum districts of Boston know how comparatively rare it is to see an Italian drunk in the street. Nevertheless it appears that alcoholism, defined as above

(and not including the indulgence in light Italian wines which were not considered in this tabulation,) is more frequent among the Italians than among any other race but one. Conversation with Italian physicians confirms these figures. It will be observed that the Jewish race which has the best record in relation to venereal disease has also the best record in regard to alcoholism, though the discrepancy is not here so marked.

IV. THE ABUSE OF TOBACCO.

In computing the number who use tobacco excessively (XIII) I have used the following criteria. I have considered as excessive (1) the use of more than one ten-cent plug of tobacco a week; (2) the smoking of more than three cigars or twelve cigarettes a day. However erroneous this criterion may be, it is at least applied universally to all the cases here considered.

Judging by this criterion it appears that from one-fifth to one-fourth of all the races here considered use tobacco to excess (see Table XIV). There is here little discrepancy between the different races, the Irish having slightly the worst record and the Italians the best.

V. RACIAL CHANGES IN HOSPITAL POPULATION. (M.G.H.)

In connection with the figures just presented, it may be of some slight interest to tabulate the movements of the different races from year to year in the population of the Massachusetts General Hospital, dealing now only with the Out-Patient Department. In Tables XV, XVI, XVII, XVIII, XIX, XX, XXI, XXII, it appears that there is a very significant increase in the number of Italians and Jews—as would of course have been expected— a corresponding *decrease, absolute as well as relative,* in the number of Irish, while there is an equally marked decrease in the percentage of Germans and of Negroes, with a slight and relatively insignificant increase in the number of Scandina-

vians and French Canadians. I have no explanation for the fluctuations in the number of Americans and English.

V.—DISEASES WHICH APPEAR TO BE BECOMING LESS FREQUENT AS TIME GOES ON.

Taking the figures in a wholly uncritical way, it would appear that there has been a decrease, in fact a striking decrease, in the amount of *acute nephritis*, *sun-stroke*, *chlorosis*, and *malaria*. Analysis of these figures shows however that, except in relation to that of malaria, the decrease is much more apparent than real. Nevertheless even these apparent and fallacious changes have a certain interest.

TABLE XXIII.

ACUTE NEPHRITIS.

YEAR.	CASES.	YEAR.	CASES.
1899	9	1905	4
1900	6	1906	6
1901	10	1907	5
1902	10	1908	5
1903	6	1909	3
1904	4	1910	1

ACUTE NEPHRITIS.

Taking for example the cases of acute nephritis in the Mass. General Hospital, Table XXIII shows *apparently* that there has been a very marked falling off in the number of cases of acute nephritis at this hospital. In preparing this table I first studied the record of every case diagnosed as acute nephritis. Those which were so diagnosed, but in which there was an obvious enlargement of the heart and an increase of blood pressure, have been thrown out. Presumably these are chronic cases. But for these omissions the discrepancy between the large number of cases formerly diagnosed as acute nephritis and the small number which now receive that diagnosis, would have been much more

marked than it appears in Table XXIII. Nevertheless I do not believe that Table XXIII represents the actual facts, for it is only within the last few years, practically since 1907, that our records show careful and repeated estimations of blood pressure as a matter of routine in all cases of nephritis. In the years previous to that time, one misses this all-important criterion in the distinction between acute and chronic nephritis, and has to rely wholly upon the history, the urine, and the dimensions of the heart as recorded. All of these criteria are, in my opinion, far inferior in value to the record of blood pressure. The history and the urine are notoriously insufficient for diagnosis, since a case of chronic nephritis may be absolutely devoid of symptoms for a long period of years, finally flaring up into what we call an "acute exacerbation" which for the first time leads the patient to call himself sick and to consult a physician. The urine likewise during such an acute exacerbation may be indistinguishable from that of acute nephritis, and, especially if the patient is fat or œdematous, the measurements of dimensions of the heart may be quite unreliable.

I believe then that this decrease in the number of diagnoses of acute nephritis represents a greater degree of accuracy in our clinical methods, and not a change in the incidence of disease. I believe that in the clinical material available at the Mass. General Hospital there are actually seen almost no cases of acute nephritis, while on the other hand the number of chronic cases is always large. It must, of course, be borne in mind, that we admit at the Mass. General Hospital no cases of scarlet fever or of diphtheria, except by accident or mistake, and that the puerperal infections are treated elsewhere. The few genuine acute cases of acute nephritis which we see are those occurring (*a*) in connection with pneumonia and other pneumococcous infections, and (*b*) with the acute pyogenic infections, originating in the throat and involving the joints and the endocardium.

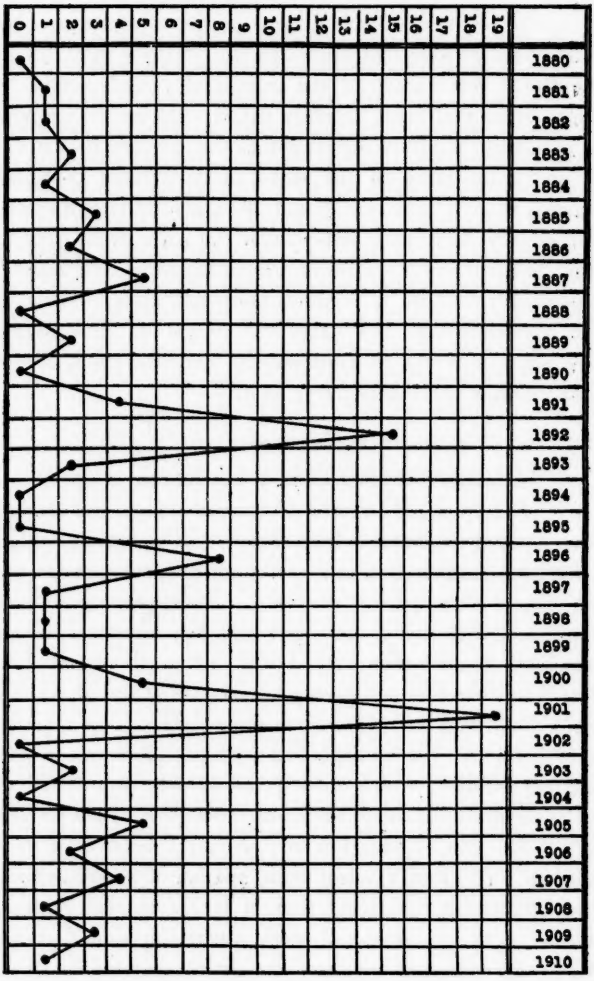
CHLOROSIS.

On the face of the reports it would appear that chlorosis is rapidly becoming a rare disease in this country, and indeed there are excellent clinicians who believe that this is the fact. But a study of the individual records seems to me to prove that cases formerly called "chlorosis" are now in increasing numbers diagnosed as "*secondary anæmia.*" If one enumerates all the cases of apparently causeless anæmia in girls between 16 and 23, one finds that they are not rapidly decreasing but remain the same from year to year, but that fewer and fewer receive the name "chlorosis," presumably because it becomes more and more obvious to the physicians that this name was always an unfortunate one, since it refers to a supposedly greenish color of the face which is practically never seen.

SUN-STROKE.

In the summer of 1892 when I was interne at the Mass. General Hospital, we treated 15 cases of typical sun-stroke, with coma, temperatures from 109° to 115° rapidly falling after the administration of ice rubs and ice packs, and in the majority of cases, prompt recovery. Remembering my own experience with this disease, and with the number of cases which we treated that year, I was very much surprised to find that there had not been a single case at the M. G. H. since 1905, and only three cases since 1901. This remarkable fact could not be explained by any differences in our summer climate, which during the decade from 1900 to 1910 certainly furnished some summers quite as hot as that of 1892. The explanation however is, I believe, a two-fold one: (1) that a considerable number of these cases are now treated at the Relief Station in Haymarket Square, which began its beneficent work about the time that the M. G. H. ceased to have any sun-struck patients to treat. (2) The other explanatory factor appears to me to be the

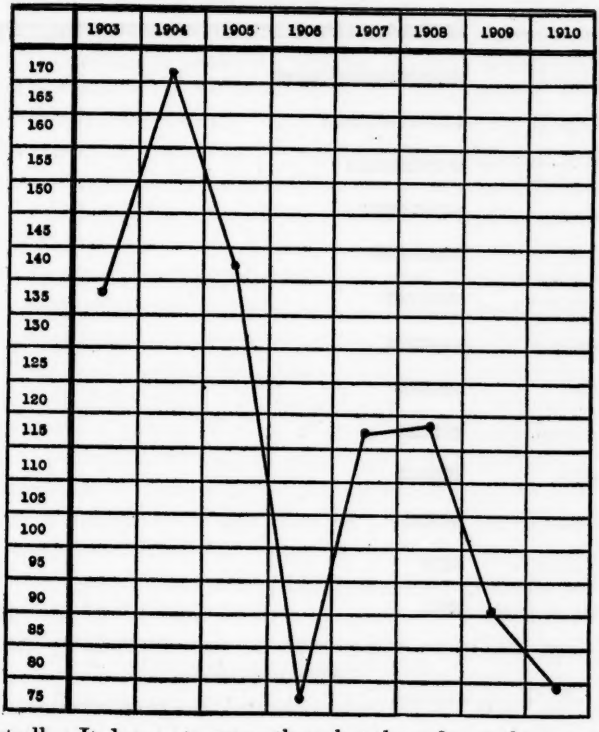
TABLE XXIV.—SUNSTROKE.



very striking variation in the number of cases from year to year when one looks over a long period.

Thus it appears in Table XXIV there have always been single years in which the number of sun-strokes was very large, and periods in which the number was small, or none

TABLE XXV.—Malaria.



at all. It does not appear then that these figures have any significance or importance.

MALARIA.

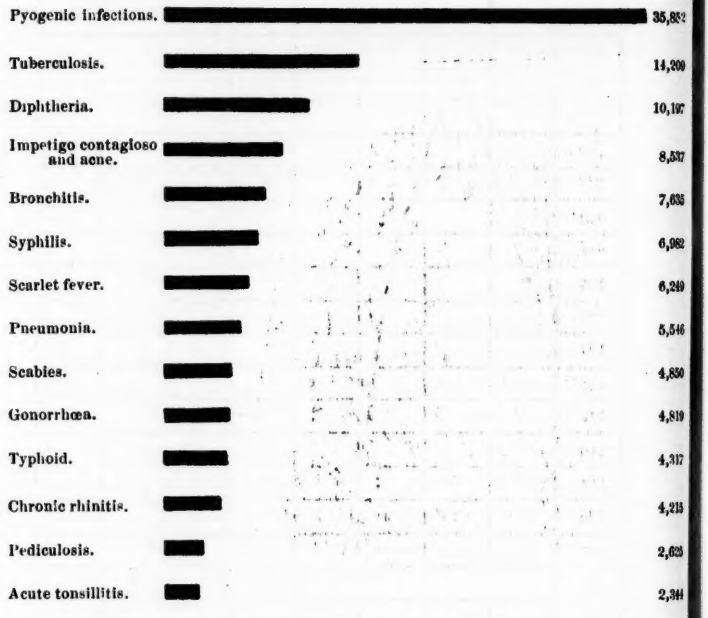
Table XXV contains the number of cases treated at the Mass. General Hospital and the Boston City Hospital, in-

cluding both the ward cases and the out-patient cases, for the past eight years. I think the clinical experience of any one of us would have led him to expect just such a change.

TABLE XXVI.

Relative Frequency of Infections.

8 years at Massachusetts General, Boston City and Long Island Hospitals.



THE RELATIVE FREQUENCY OF THE DIFFERENT INFECTIOUS DISEASES.

In Table XXVI, I have presented the figures corresponding to the relative frequency of the different infectious diseases, as observed at the Boston City Hospital, the Mass. General Hospital, and Long Island Hospital, during the past eight years. A few words should be said in relation to the term "*pyogenic infections.*" Under this phrase I have included

all cases diagnosed as pyæmia or septicæmia, all cases designated as local sepsis ("septic hand," "septic foot," etc.), all local abscesses and boils, except those abscesses originating in the appendix; I include also all the cases of acne and impetigo contagiosa. I take it as generally admitted that all of these diseases are due to pyogenic cocci, the activities of which are made more than usually pernicious by a temporary diminution of the individual's power of resistance.

It is certainly questionable whether the cases of appendix abscess should not be included in the same group, and whether the fundamental causes of appendicitis are not the same as those of other local pyogenic infections. Had the cases of appendicitis been included, the figures listed under "pyogenic infections" would have been increased by about 5,000.

The other items of the table can offer little comment. Any one would have expected that tuberculosis would come next, although I suppose many would have unthinkingly put it first, forgetting the omnipresence of pyogenic organisms and their results. To many I think it will be surprising that the number of cases of syphilis is so much larger than the number of cases of gonorrhœa. This is doubtless due in part to the fact that so many cases of gonorrhœa remain untreated or do not come to a hospital for advice. Nevertheless I think there cannot be so large a preponderance of gonorrhœa over syphilis as has been generally assumed to be the case.

SUMMARY AND CONCLUSIONS.

1. In a hospital population of about 8000 males over 18 years old, all of whom were carefully questioned, 35% gave a history of gonorrhœa, and 11% a history of syphilis, at some period of their lives. These histories and the resulting percentages are believed to represent approximately the truth about an average sample of Boston's adult males.

2. The Americans, Irish, English and Italians have about the same amount of both these venereal diseases. The Jews have about ⅓ as much syphilis and ½ as much gonorrhœa as the other races.

3. The history of an excessive consumption of alcohol (defined in the text) was obtained in 32% of 8356 patients questioned. The Jews show the smallest amount of alcoholism, — the "Americans" the highest percentage of total abstainers.

4. About ¼ of all the 7186 adult males who were questioned gave a history of using an excess of tobacco (defined in the text). The Italians appear to use rather less than the other races but the racial differences are less marked as regards tobacco than as regards alcohol and venereal disease.

5. The statistics relating to alcohol and tobacco are regarded as an indication of the habits of a fair sample of Boston's population.

6. Acute nephritis, chlorosis and sunstroke would appear (on the face of the records) to be decreasing. This decrease is shown by analysis, however, to be in all probability fallacious.

7. Pyogenic infections are more than twice as numerous as any other known infection (tuberculosis included).

8. Three Boston hospitals treated 6982 cases of syphilis and only 4819 of gonorrhœa in the years 1903–1910 inclusive.

ARTICLE III.

SUITS FOR ALLEGED MALPRACTICE.

By GEORGE W. GAY, M.D.
OF CHESTNUT HILL.

READ JUNE 14, 1911.

Suits at law against reputable physicians for alleged malpractice have attracted unusual attention of late years by reason of their increasing frequency and by the absence of a reasonable foundation of truth and justice in the charges usually brought against the defendant.

Litigation, like legislation, was never so rampant as it is to-day. The multitude of lawyers, the bewildering mass of statutes and the ever increasing volume of both, only tend to aggravate a condition which would seem already to be too complicated for the public welfare. The craze for legal proceedings upon any and all occasions, however baseless the charges, the desire to seek revenge for injuries, real or imaginary, the temptation to extort money without earning it, the hustling activity of a certain class of attorneys, all conduce to a state of affairs that is anything but satisfactory to the better class of the community.

In the words of one of the Chief Justices of Massachusetts, "The state to-day is not only furnishing a resort to justice, but it is furnishing a field for business speculation. What a large and growing number of litigants want is not a resort to justice, but a resort to chance; not the establishment of truth and the enforcement of whatever rights based on the truth have been infringed upon, but a verdict, or a decision obtained from ignorance, prejudice or favor, and the enforcement of that verdict or opinion. The courts of

equity are resorted to in business complications; the courts of law as a means of transferring A's money to B's pocket!"

As a result of these and other conditions, our courts are overwhelmed by the enormous volume of business awaiting their attention. Massachusetts has 127 judges and 170 special justices in its judicial system. On June 30, 1909, there were pending in the Superior Court of Suffolk County 14,703 cases, of which nearly 4,000 had been entered during the year just passed.

A recent examination of the records of the Superior Court in this state shows that the average time from the entrance of a suit to its first trial is over two and a half years, a condition that is hardly consistent with one's idea of the prompt administration of justice. As a matter of fact the prompt and speedy administration of justice in our courts is seldom possible at the present time, and the conditions show no sign of improvement. To such a degree of congestion has the condition obtained in many states, that the President of the United States, the American Bar Association and many of the acute legal minds of the country are trying to devise some method of correcting this unfortunate state of affairs.

In no direction have claims and suits at law increased to so great an extent in recent years, as in cases of damages for alleged personal injuries. They have grown to such enormous proportions that the time and attention of some of our courts are largely occupied with them alone. In Suffolk County during the year ending in June, 1909, out of 1146 cases actually tried in the Superior Court, about 660 cases, nearly 60 per cent., were for damages for personal injuries.*

For the past ten years about 8000 claims have been brought annually against one of the principal public service corporations in this vicinity. A very large proportion of

* Hon. William Schofield, Boston Medical and Surgical Journal, Dec. 30, 1909.

these claims have been for personal injuries. The trial list of the Superior Court in Suffolk County in one week recently contained 267 cases, of which 121, or nearly one half, were tort cases against this corporation. The annual expense to the company on this account amounts to about $600,000!

The character of many of these claims is plainly shown by the frequent failure of the plaintiffs to secure verdicts, despite the proverbial leniency of the modern jury in their favor, as against corporations. Not a few of the claims have no foundation in fact or in justice. This is indicated by the diagnoses which are so frequently presented. For obvious reasons, impotence figures less frequently than formerly, as a result of litigation injuries. Neurasthenia, rheumatic arthritis, spondylitis deformans, and various other obscure and debatable affections are favorite diagnoses and give the honest expert no little trouble in separating the true from the spurious cases. Thus far there seems to be no relief from this deplorable state of affairs, nor are the prospects for such relief encouraging.

In view of existing facts and conditions, it is not strange, although deplorable, that reputable members of the medical profession are called upon at times to defend themselves against unjust charges of neglect and incompetency. However faithful and efficient may be the physician, he is always liable to an action at law for any professional service whatever that he may render to the plaintiff. He may be a victim of the whim, the caprice, or the spite of the most vicious and depraved rascal out of prison. He is in no way protected from a suit by the fact that the mode of treatment pursued in any case was the one accepted and approved by the leading authorities, or that from the nature of the disease or injuries, no known method of treatment could have improved the results obtained. These facts do not protect a physician from an unjust, vexatious and expensive suit at law in any case and under almost any circumstances.

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Having rendered faithful, skilful service through a long and perhaps critical illness, and having obtained the best result possible under the circumstances, nothing can be more disheartening, more aggravating, more demoralizing to one's faith in human nature, than a suit for malpractice. With no just cause for dissatisfaction, the most vindictive spirit is not infrequently shown toward one who has done his utmost, as much in fact as it was possible for anyone to do, for his welfare. Yet the conditions were such that in spite of the services rendered the results were disappointing to the patient and physician alike, results due in no sense to a lack of intelligent effort on the part of the latter, but to conditions over which he had no control and for which he is not, and should not be held, responsible in any way.

To assert that physicians never make mistakes under any circumstances is to claim a degree of perfection which no profession nor vocation possesses. From the nature of our calling and of the conditions with which we have to deal, an occasional error is inevitable. The wonder is that the mistakes are so few. Most of them occur under circumstances that go far to mitigate censure and lessen blame. Nature's processes are often obscure and the danger signals may not be in evidence until too late to avert disaster. Many complications are sudden in their onset and give no warning of their approach, nor are they frequent enough to be anticipated under ordinary circumstances. As the Supreme Court of a neighboring state has well said, "the medical practitioner is necessarily surrounded by doubt and uncertainties and unavoidably exposed to errors and mistakes, which may well furnish a satisfactory explanation of unfavorable results."

Another element that is frequently suspected, but unfortunately difficult to establish, is the contributory negligence on the part of the patient. We all know by experience how negligent many patients are of the physicians' orders.

Not infrequently they take the management of their case into their own hands during the absence of the physician, or take advice of an ignorant friend, as being more agreeable or involving less trouble, than the one of the moment. Whatever is done, or by whom it may be done, the physician is held responsible for the results, or the outcome, regardless of right or reason. He has no redress and oftentimes is unable to establish his innocence.

The members of the medical profession stand ready to make reasonable amends for errors and to adjust proper claims. They strongly object, however, to being mulcted and fleeced by sharpers and unreasonable people, as is attempted in a vast majority of the malpractice suits of the present day. The physician has some rights, among which is the right to defend himself against false charges and claims for results over which, from the nature of things, he had not control, and for which, therefore, he cannot in justice be held accountable.

Malpractice suits are brought from various motives, as for example, to extort money from the defendant, to escape the payment of his bill, to avenge the disappointment at the termination of the case, to deliberately injure the reputation of the defendant out of spite and to attract the notice and sympathy of friends and neighbors. The sanctimonious excuse of desiring to protect others from the incompetency of the defendant is occasionally offered as a reason for bringing suit. It is doubtful if a patient of his own motion often brings an action for damages in these cases. More frequently they are instigated and encouraged by wofully mistaken friends and neighbors, who, knowing little or nothing of the real facts, are very free to proffer advice which carries no responsibility, and is generally the offspring of impulse and the litigious spirit of the age, rather than of reason and common sense. A very important factor in many of these cases is the perniciously active attorney, who is

constantly on the watch for opportunities to instigate and foment claims of all sorts with little regard to right and justice. Given, a shyster lawyer, a disgruntled patient and a medical adventurer, and we have all the factors of a prolonged and vexatious persecution of an innocent physician, whose chief misfortune is his connection with the case.

Indiscreet remarks or opinions by physicians are occasional causes of malpractice suits. It behooves us to beware of hypothetical cases. Too often they have an ulterior motive, which may give rise to no end of trouble.

A case in point occurred in this vicinity many years ago in which a leading physician was consulted as to the proper treatment of a pretended hypothetical case. The opinion, which was obtained and paid for, being favorable to the plaintiff, a suit was immediately brought against a prominent physician for alleged malpractice. After dragging along in the courts for three or four years, the jury finally disagreed. The defense cost $2000 at a time in the defendant's career when he could ill afford the money, to say nothing of the anxiety and trouble attending the event. By a little consideration and forethought the whole vexatious business could easily have been avoided.

A suit for malpractice against one of Boston's ablest surgeons was instigated many years ago by a physician, who had been saved from a similar experience in the past by this very surgeon's influence with a dissatisfied patient. The young man had made the not very uncommon mistake of calling a broken hip a contusion. The surgeon above alluded to set him right, not only in his diagnosis, but in his relation to the family, so far as any legal action was concerned. In the present instance the demands of the plaintiff were met with the emphatic declaration, "Millions for defense, but not one cent for tribute." That ended the matter.

This surgeon had a peculiar experience at the commence-

ment of his career, which is worthy of mention. At the conclusion of a suit against him for alleged malpractice in which he received a verdict, the following resolution was handed him by the foreman of the jury that decided the case: —

"Resolved, That in the opinion of this jury, Dr. Thorndike exercised the best skill and judgement in the surgical operation in question, and that he is entitled to the entire confidence of the community in the practice of his profession, as a surgeon and physician."

This resolution was signed by every member of the jury. Not only that, but the plaintiff's counsel, two prominent members of the Suffolk bar, also wrote the doctor, saying that in view of the evidence produced at the trial relating to his skill and ability, they were well satisfied with the verdict. This event occurred nearly sixty years ago, when malpractice suits were much less common than they are to-day.

Suits brought for the purpose of evading payment of the physician's bill may generally be successfully met by prompt legal action for the collection of the same. The white feather under these circumstances will be taken as a confession of guilt and the advantages thereof pushed to a conclusion. A counter suit is probably the most effective method of meeting the situation.

Not infrequently a malpractice suit is brought against the consultant, either separately or in conjunction with the attending physician. This is especially noticeable when the former has attachable property, or the reputation of being wealthy. "The doctor is rich and it won't hurt him to give the plaintiff something, whether he is guilty of the charges or not!" To the credit of the courts be it said that these verdicts are not infrequently set aside.

Holding the consultant equally responsible with the regular attendant for the treatment, especially if the patient be

seen but once by the former and at a time when the mode of treatment had long ceased to be a factor in the result, may be good law, but in many instances it is neither justice nor common sense. It often results in flagrant injury to innocent persons. It may subject physicians of the highest skill, who have rendered faithful and intelligent service, to the necessity of defending themselves in blackmailing suits against charges having no foundation in fact, or for conditions over which they never had any control, and for which they are in no way responsible. It encourages false claims and fake suits. It invites all sorts of tricks and subterfuges to extort money, to gratify revenge and to injure innocent persons. Wealth, real or reputed, invites attack. Visible property is especially tempting to adventurers of all sorts. Under certain conditions it is not an unmixed blessing, as many members of our profession have found by an aggravating experience.

The prevailing freedom of legal procedure is doubtless based upon the constitutional right which every individual possesses of suing anybody or his estate, for anything regardless of any just grounds for such action. The principle is right, but it would seem desirable to have the practice under some sort of supervision by which a large number of the trivial or baseless suits might be avoided. The English courts manage this business much better than ours, as will appear later in this paper.

The time limit during which these malpractice suits may be brought should be curtailed. Under the present laws an adult has six years in which to commence action, while in the case of minors, claims can be brought at any time during their minority. This latitude would seem to be needlessly ample, in that it may result in injustice to the defendant, or his estate through death, the absence of witnesses, as assistants, nurses, etc., and the various changes of time and events. As the ultimate results of injuries and diseases

can almost always be determined within two or three years, it would seem reasonable and just that the time for bringing these claims might be reduced to three, or at the most to four years. No one would be likely to suffer hardship thereby, and the defendant would in consequence be placed more nearly upon a level with the plaintiff in the case.

The medical profession in an adjoining state has been passing through a severe epidemic of malpractice suits of late, as is indicated by the remarks of a learned judge upon a recent occasion to the following effect: —

"Just now our courts are abounding in malpractice proceedings against members of your profession, and verdicts are following each other in swift and justly alarming succession against the most distinguished of your number and for amounts, on account of alleged imperfect repair, in excess of the damages awarded for the entire injury against corporations in no more serious cases."

It has been currently reported that eighteen cases have been entered in the Supreme and Superior Courts of that state, or are being prepared therefor. Several trials have already taken place with varying results. While some of the verdicts have been satisfactory to the better classes in the community, others have been of such a character as to disturb one's respect for, and confidence in, juries and jury trials.

An action was recently brought against two reputable physicians for alleged malpractice in the treatment of a broken hip. The damages were laid at the modest sum of seventy thousand dollars! The patient had been treated in a hospital in the usual manner, with extension, fixation, etc., and had made a good recovery. The limb was shortened three fourths of an inch and was fairly useful.

Ostensibly the suit was brought to obtain compensation for pain, lameness and shortening of the limb, the three factors that always attend this injury under any and all circumstances, whatever may be the mode of treatment. To

the credit of the profession be it said that no reputable physician appeared in court as an expert for the plaintiff, a fact that was noticed rather emphatically by one of the plaintiff's counsel.* Two of our most accomplished surgeons examined this patient, but neither appeared at the trial in person or by deposition. Eight lawyers were employed upon the case and the trial lasted nine days. Verdict for the defendants. Their defense cost them about $3000.

The interest shown in this trial by the best people in the county, together with the loyalty manifested by the medical profession, must have convinced the plaintiff's counsel that, instead of this action lying against the defendants, it lay against a large portion of the community. In fact it would not have been easy to find a physician in that vicinity at that time, who would accept service in the care of a fracture of the hip, so thoroughly aroused were the physicians by the injustice of the charges brought against their fellows. They realize, as never before, the importance of protection against a similar exasperating and expensive experience.

Many years ago a suit for alleged malpractice was brought against the estate of an able physician in this city for a ruptured perineum occurring, as it was claimed, at confinement. Although the accident was not a recent one, yet the suit was brought after the death of the physician. As might be expected, the heirs were handicapped in their defense, and notwithstanding the fact that several of our leading physicians testified to the groundless nature of the claims, the estate was mulcted in several hundred dollars, a result that doubtless would not have obtained, could the physician have been present to defend himself.

The Nestor of the medical profession in this vicinity today had a suit for an abdominal hernia following a laparo-

* The plaintiff's principal "expert" has achieved a certain notoriety in this sort of business, but belongs to no medical societies for the reason, as he said during his cross-examination, that "Their morals do not come up to my standard!"

tomy hanging over him in court for nine years in charge of a lawyer noted for his unscrupulous tricks and obstinate persistence in trying cases. To avoid post mortem complications, the defendant insisted, contrary to the advice of his counsel, upon having the case pushed to a trial. When it was finally reached, the plaintiff failed to put in an appearance, and the matter was closed.

A suit for a large amount was recently brought against an able surgeon for giving an opinion as to the proper treatment of an old fracture, under the following circumstances: A physician brought a lad for advice in relation to a slight injury to the wrist, which had occurred within a day or two. On making an examination the surgeon noticed a deformity, and upon inquiry found it to be due to a Colles' fracture some weeks old. His opinion was incidentally asked as to the advisability of making attempts to correct the deformity. In view of the time that had elapsed since the injury and of the fact that the functional recovery was satisfactory, further interference to correct any deformity was not advised. The consultant saw the patient only once, as described above, but six years later a suit for alleged malpractice was brought against him for giving an opinion as above! (This defendant has been dropped from the suit on the plea of a misunderstanding!)

A well known surgeon has recently had a verdict of several thousand dollars rendered against him in a jury trial for alleged malpractice in the treatment of a fracture of the shaft of the femur. It was a case of delayed union and was under the care of the defendant during the first month of the treatment only. On turning this patient over to his successors in the hospisal service, a careful examination in consultation showed the fragments to be in good position, the limb was straight and all seemed to be going on well, except that no attempts at union of the fragments were in evidence. The defendant had nothing more to do

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with the patient, as his term of service had expired by rotation.

Two years later a suit was brought against this gentleman for alleged malpractice with the result above mentioned. If this was not a miscarriage of justice, it might be worth while to demonstrate what is! *

These and other cases known to us all may well suggest the query as to how long it may be wise for the consultant to keep in touch with patients seen with other physicians. Said a prominent consultant in this vicinity many years ago to a young physician on being called by him to see a fracture: "Doctor, with your permission, I will see this patient with you occasionally until he recovers." The suggestion was highly satisfactory to all parties. The astute consultant recognized his responsibility and took proper measures to protect not only himself, but also the family physician from any possible complications. Of course the consultant cannot prevent the bringing of a claim against himself, or the family physician, but by having an oversight for a certain period, he may be able to detect and perhaps avert untoward events that might lead to future trouble. While circumstances must guide one's action in each case, yet the idea is of sufficient importance to justify serious consideration during the first ten or fifteen years of practice.

A brakeman had his arm crushed by a freight car. He was taken to a hospital, and was told by the physicians that if an examination under ether corroborated the external appearances, he would have to lose his arm to save his life. He replied, "You doctors know best what to do, and I leave it to you three doctors to do whatever you think best." This was said in the presence and hearing of three physicians and a nurse. Under these circumstances the anæsthetic was given and the arm found to be so badly mangled

* The Supreme Court has set the verdict aside and ordered a new trial.

that six or seven inches of the bone were removed by the fingers alone. This condition of the bone, together with the extensive destruction of muscles, vessels and nerves, rendered an amputation absolutely necessary. The operation was done, and a prompt recovery followed.

Instead of bringing an action against the railroad company for the injury, he brought suit against the physician in the sum of $25,000 for amputating the arm without his consent, as he claimed, and against his express orders that it should not be taken off! The jury was out twenty-five minutes and brought in a verdict of $12,000 for the plaintiff! Fortunately for him and for the cause of justice, the verdict has been set aside by the Supreme Court.

A few more verdicts of this sort may lead surgeons to consider the advisability of taking advice *before* taking charge of these cases, rather than run the risk of being obliged to do so *afterwards*. These complications might also be occasionally avoided by resorting to the custom in vogue in some of the hospitals in relation to autopsies, none being allowed except upon the written consent of the proper authority.

Every one knows it to be a well established fact that a person in his right mind has a right to decide as to whether any operation shall be performed upon himself.

In cases in which the patient is unconscious, or unable from any cause to decide these matters for himself, when coming under the care of a physician, the latter would be justified in performing any operation demanded by the emergency, provided no one having authority should be present and refuse consent thereto. The physician should, however, protect himself by a consultation, if possible, and it might be well to explain the conditions and the reasons for the proposed operation to those present. The entire transactions together with the names of those present should be a matter of record.

In those cases in which no provisional arrangements have been made previous to the administration of the anesthetic, the surgeon's natural consideration for his patient's feelings and comfort would prompt him on finding unexpected conditions demanding further operative proceedings, to do then and there whatever might be necessary, rather than allow the patient to rally from the influence of the drug and thus be obliged to subject him to a second etherization. By so doing, however, the surgeon would render himself liable to an action for damages for assault in not first obtaining consent to the necessary operation. The physician would be justified in doing such operations only under extreme circumstances, and with a clear comprehension of his own liability as well as of the disposition of the patient.

Another possible source of embarrassment to the physician lies in the signing of commitment papers to an asylum or elsewhere. Occasional suits are brought against physicians for alleged false commitments and also for detention, as in the following instance, which occured in this vicinity a few years ago.

A man was arrested for abusing his wife, who was ill with pneumonia, and for assaulting the nurse, who was caring for her. Two physicians were called to decide as to the prisoner's sanity. Upon declining to commit the man as being insane, they were requested to sign papers authorizing his detention in the station house till morning, when he could have an examination by experts. The physicians were assured by the captain of police that this custom was common, that otherwise he could not hold the prisoner, who was evidently unsafe in his present condition to be at large, and that furthermore, by signing detention papers, they would incur no risk whatever of a suit for false arrest, etc. Under these circumstances the papers were signed and the prisoner was detained over night.

The experts having decided the next morning that the

man was not insane, he was discharged, and at once brought an action against the physicians and the nurse for false arrest in the sum of $90,000. The case came to trial in about two years, the defendants having in the meantime very properly refused all overtures for a settlement. The jury gave the defendants a verdict, but their defense cost them about $1600.

A physician in a neighboring state, on being threatened with a suit for a bad fracture of the wrist, enticed the young man into a medical meeting and had him examined by several of the leading men of the city. They each and all expressed the opinion then and there that the result of the treatment was as good as could have been expected under the circumstances; that the doctor was to be complimented, rather than censured for the excellent result obtained. On leaving the society the young man was informed that his doctor had no attachable property, a fact that might be of interest to his attorney. That ended the case and nothing more was heard of it.

Of course there is another side to this matter, and no one will deny the fact that malpractice suits are justified under certain conditions, of which the following is an example: Some years ago the writer was requested to serve as expert in an expected suit for malpractice in a case of unrecognized dislocation of the humerus. It appeared that an old gentleman had fallen on the ice and injured his shoulder. A physician was called who was not competent to make a diagnosis, nor did he ask for consultation, or aid of any sort, but treated the patient for several months for a sprain or a contusion. As no improvement was evident during this period, further advice was then sought and the real nature of the injury determined for the first time, nearly a year after the accident.

The patient very naturally refused to pay the physician's bill, and a suit for its collection had been entered. A

counter suit for malpractice was threatened and the doctor was hunting for an expert to help him defend his case. For obvious reasons the writer refused to have anything to do with it. The physician was totally unfit to take charge of such cases and, furthermore, he was inexcusably negligent in failing to call a surgeon immediately to make a correct diagnosis and reduce the dislocation. As the doctor had no defense, either in law or in justice, he was advised to withdraw his suit for his bill, and to settle the matter as best he could.

It should be said that cases similar to the above are rare in these days of progressive surgery. They come under the head of malpractice, pure and simple, and are inexcusable under all ordinary circumstances. This paper makes no plea in defense of such work, but confines itself to those cases in which there is little or no just cause for complaint, as to the treatment.

It would be difficult to devise a scheme, or a mode of procedure, more conducive to the instigation and presentation of bogus claims, than is the so-called contingent suit of the present day. It is a game of chance, a gamble, having stakes that are especially alluring, when the defendants are timid, or rich, or both. The bait is especially tempting to the impecunious classes in the community and particularly so to a certain class of lawyers and doctors.

These contingent cases are often taken "on spec," as the common phrase has it; no verdict, no expense to the plaintiff, *i. e.*, if he has nothing to pay with. He is encouraged by the attorney to enter his claim and is assured of success, either by settlement or a trial. The attorney often assumes the costs of the trial, depending upon a verdict for his compensation. It is currently reported and believed that in cases of a favorable verdict, the amount is frequently divided equally (?) between the lawyer, the doctor and the plaintiff. This method of dividing the spoils is open to doubt.

Be it said to the credit of the courts, however, that not infrequently these unjust verdicts are set aside, as being contrary to the evidence, to common sense and to the cause of justice.

The frequency of these contingent suits has not a little to do with the overwhelming number of cases that burden our courts and clog the wheels of justice. Corporations are the especial prey of these adventurers. The fact that some of them are put to an annual expense of hundreds of thousands of dollars in defending themselves against these claims, affords an indication of the magnitude of the practice. To such a degree of abuse and imposition has the custom obtained in many sections of the country, that a movement for its regulation and control has been inaugurated by the American Bar Association, and by the courts in several states.

The cause of justice would be very greatly advanced in many instances, if in the discretion of the court, bonds were required of the plaintiff to defray the costs of the court in case of failure to establish a claim. This practice is occasionally resorted to, and it would seem wise to make use of it more frequently in the interest of fair play.

As conditions are at present, suit after suit may be brought on the most senseless charges, in evident spite, to the great detriment of the defendant. There should be some sort of supervision by the courts, or otherwise, for the purpose of preventing fictitious claims coming on trial, thereby wasting the money of the state and the time and strength of its agents. This is done in England to the manifest advantage of the state and of justice. The better class of attorneys in this country are keenly alive to the situation, are agitating reforms along various lines, and in time will doubtless accomplish certain results in the way of improvement.

Of course the writer is well aware of the argument that

will be presented in opposition to this suggestion, namely, the interference with personal liberty, etc.; yet he ventures the opinion that in flagrant cases of persecution, the only sort in question for the moment, the matter of personal liberty is of much less importance than is that of personal justice. In the opinion of many of our brainiest and most reliable men, we have altogether too much liberty in certain directions, and it is more than possible that unrestricted admission to the courts may be one of them.

Could the courts of this state have the common law power restored to them whereby they would be authorized to charge the juries upon the facts, which of course includes opinion evidence, many of the evils of the present day trials, that are a disgrace to both professions, would be corrected to a marked degree.

The education, the training and the experience of our judges can but enable them to estimate the relative value of expert testimony far more correctly than can the ordinary, inexperienced juryman. This fact is demonstrated beyond all question in the English courts. Under the present custom and law in this state, the courts are not allowed to explain the character of the evidence, nor to comment upon it in any manner for the purpose of aiding the jury in arriving at a just conclusion as to the merits of the case in hand. In consequence of this limitation, the courts may and sometimes do advise the juries to ignore the evidence of the experts altogether and base their verdict upon other and less contradictory evidence.

Repeated efforts to restore the common law power to our courts have been made before the legislature in this state for the past thirty years, but thus far without success. The Federal courts, as well as those in some of the states, have it, and all should have the power in the interests ot truth and justice. The New York courts have the authority, but strange to say, they seldom use it. It is within the range

of possibility that the character of some of their recent trials might have been somewhat improved by a judicious use of this power. The fact is worthy of notice that the judges in that state are elected, not appointed, as they are in Massachusetts, thus making the position a political one, and possibly subject to some of the undesirable influences pertaining to many political offices.

In the matter of court procedures, it would seem that the American Bar might well take a lesson from the English Courts, which "stand as the leader of the world in swift and accurate justice." It is the business of courts to see that all parties at suit obtain reasonably exact and speedy justice. The English courts accomplish this object. The American courts in this part of the country do not. The reasons for this difference are not far to seek.

"The English judge takes a very active part in the proceedings and directs the trial at every stage. If he is satisfied at any stage of the proceedings that the evidence presented is not sufficient to warrant a conviction, he may stop the trial and direct a verdict of acquittal." He may call and examine witnesses of his own motion independently of the counsel with a view of bringing out more clearly the facts in the cause in hearing.

In his address to the jury he reviews the evidence in detail, expresses his opinion of the witnesses and of their testimony, separates the material from the immaterial, thus placing the most important points before the untrained jury in such a manner that they can readily understand the main issues at stake, and usually are enabled thereby to render a speedy verdict, oftentimes without leaving their seats.*

Could our courts have this power and use it properly, much imposition and useless expense would be avoided in these malpractice suits, as well as in many others. A reform along these lines is devoutly to be desired.

* Jour. Am. Institutes Criminal Law and Criminology, Jan. 1911.

The contrast between the English and American methods of procedure is well illustrated in the Tucker and Crippen cases of recent date. Tucker's trial took place seven months after his indictment, nine months after his arrest, and lasted twenty-two days. He was executed two years and three months after his arrest, or eighteen months after his trial.

On the other hand Crippen was indicted five weeks after his arrest, aud he was tried inside of two months. The trial lasted four days. The jury was impaneled in eight minutes, and the verdict rendered in twenty-nine minutes. An appeal was taken, but promptly disposed of, and five weeks after the trial began, and four months after the crime was committed, he was executed. "The trial was conducted in an orderly, dignified manner, and with every regard for the rights of the accused." These facts would seem to show the superiority of the English over the American methods of court procedures.

While malpractice suits against reputable physicians may and generally do give rise to an infinite amount of trouble, anxiety and no little expense, yet the writer cannot believe that they do much permanent harm to their reputation, or to their business. Character stands for much in our profession, and once it is established upon a firm foundation the respectable physician need have little fear that in these days of crazy litigation, a suit for alleged malpractice of the sort under consideration will undo the work of years. The people have learned by experience and observation to trust him, and that confidence is not to be disturbed by these tricks of the law. Not a few of our most accomplished physicians and surgeons have been subjected to these trying ordeals with no detriment to their character, to their reputation, or to their business.

Conscientious physicians render the same faithful care and skill to their hospital patients that they do to their private ones, and yet not infrequently malpractice suits are

brought in the name of charity patients. The hospital, as a rule, cannot be sued, but the hospital physician, who does the work gratuitously and runs all the risk of censure and suit, can be, and he has no redress at law. Too often he is obliged to pay the expenses of his defense, which in addition to the anxiety, the interruption to his ordinary life and and all that that means to himself and his family, is no small matter, as many physicians know to their cost.

The writer much regrets that he is unable to suggest any escape from this unjust and aggravating state of affairs. So long as there are ungrateful patients and pernicious lawyers and doctors, physicians, however accomplished and renowned, must run the risk of being haled into court upon the most unjust charges, and put to the trouble and expense of defending themselves, their reputation, their character and their bank account, if they be so fortunate as to have one, against the blackmailers and "ambulance chasers" that infest every community.

And this brings us to the consideration of the methods of meeting unjust claims for malpractice, with which this paper has chiefly to do. Shall they be compromised and settled, or shall they be contested in court? The writer has no hesitation whatever in advising that, as a rule, they should be contested to the last degree. Experience has shown this to be the best policy. Properly managed, few cases will be lost to the defendant.

Settlement of these claims encourages imposition and extortion. The principle is wrong and the practice is worse. The temptation to obtain money without earning it is too strong to be resisted by a large class in the community, and self-protection demands a resort to strenuous measures to counteract the present day efforts to squeeze the rich for the benefit of the poor, regardless of right and justice.

If anything has been proven by experience of late years in dealing with the overwhelming mass of claims brought

against corporations in this and many other localities, it is the fact that the more readily and easily settlements are obtained, the greater the number that will be presented. For obvious reasons it was the custom at one time for corporations to settle many claims out of court, rather than contest them. The public was quick to take advantage of this policy and the claims increased to such an extent that in self-defense it was changed to the one at present largely in vogue, namely, to settle just claims if possible, but to fight the others to a finish.*

The above policy of contesting unjust claims should be advocated even more strongly in dealing with malpractice suits. They should be resisted to the last extremity.

A malpractice suit for a large amount brought against a well known physician in this vicinity some years ago, went through four trials in as many years. Efforts were made by the plaintiff's council to obtain a settlement for sums ranging from $100. to $200. The defendant, knowing that he was right, wisely and courageously refused to settle upon any terms, except a verdict. This he finally obtained. His defense cost about $6000.

One feature of this case is worthy of mention. Previous to the last trial the court ordered the plaintiff to furnish a bond sufficient to pay the costs, not only of the coming trial, but also of all the previous ones, in case she failed to obtain a verdict. The result was that nearly a thousand dollars were required to satisfy the bond. A more frequent resort to this wise procedure would go far in preventing these blackmailing schemes that are a disgrace to the judiciary of the present day.

* Soon after the policy was changed, an attorney presented a claim against one of the large corporations in this vicinity, which he offered to settle for one hundred dollars. As the claim was an unjust one, settlement was refused. The lawyer was much surprised and exclaimed, "What does this mean? It has always been understood that a claim against this company is as good as a hundred dollars in the savings bank!"

Suits for malpractice should be, and very generally are discouraged by all right minded people. The better class of attorneys hesitate or refuse to accept service therein, realizing the fact that the few just claims can usually be adjusted outside of courts, and that the other sort have little or no foundation in fact or justice. Making due allowance for human limitations, the cases are rare in which a respectable physician should be haled into court and made the victim of public criticism, censure and pecuniary forfeit. And furthermore, the instances are still more rare in which it is a physician's duty, or even in which he is justified in appearing in court as an expert against a reputable practitioner, who is defending himself in a suit for alleged malpractice. Reasonably faithful, skilful and conscientious service should receive the approbation and support of the members of our profession, rather than their active or passive condemnation. In the interests of a "square deal," of right and justice, the honorable physician should be safe with his fellows and associates.

No physician is legally obliged to respond to any call for his professional services, Common humanity compels him to respond to demands that he might not accept upon other grounds, but the law has no more control over his professional services, than it has over those of any other class of people. A doctor is not a public servant, as is a policeman, or a fireman.

Having accepted service in any case, however, the law requires three things of him, to wit:—"A reasonable degree of the learning and experience ordinarily possessed by the medical men of the time and neighborhood. Reasonable and ordinary care of the case committed to him. Exercise of his best judgment in cases of doubt." These promises he takes with him to every sick room.

Character, education and experience are the main tests of one's fitness to practice medicine. The physician is legally

and properly bound to exercise due care and skill in the treatment of his patients. Having done this, he is not responsible for the results in the case, whatever they may be.

The law does not call for extraordinary care and skill, nor does their exhibition shield the physician from the malicious attacks of adventurers and blackmailers. The most accomplished practitioner is as liable to an action for alleged malpractice as is the most disreputable pretender in the community. No qualifications suffice to protect the physician from these assaults. Care, forethought and discretion would seem to be our only safeguards.

The law gives to every one reasonably even and exact justice. Our courts endeavor to carry out the provisions of the law to the best of their abilities, and in the interest of justice. Attorneys devote their energies to the interests of their clients without much regard to abstract justice. The courts are usually as considerate of the interests of the defendants in these common malpractice cases as is possible. Could they but have the rights and privileges of their English brethren in the judiciary, we should have little reason to fear the outcome, as few of the fake cases would ever reach the jury. Even under present conditions, the verdicts in a large proportion of cases lie with the defendants. Yet to obtain them they are put to much trouble and expense, to say nothing of the public annoyance and the whole wretched business that attends these affairs.

While we cannot prevent claims being brought against us upon all sorts of unjust and unreasonable charges, yet we can do much to protect ourselves from disastrous possibilities resulting therefrom.

In the first place it is essential that the physician starts fair and right. He should undertake only such cases as he can properly manage with or without available assistance. A reasonably correct diagnosis is the basis of satisfactory clinical work and no pains should be spared to obtain it.

Prompt and repeated consultations should be requested in difficult and obscure cases for the double purpose of avoiding error and dividing responsibility. In these days of highly developed specialism it is incumbent upon the family practitioner to avail himself of the services of experts upon all proper occasions not only for the benefit of the patient, but for his own as well.

As a rule it is better that the prognosis should accompany the diagnosis. Careful and explicit explanations of the nature of serious cases, together with the complications liable to arise and their probable termination, may well be given to the patient or to some reliable person early in the attendance. This for our own protection. While the conscientious physician naturally desires to spare the feelings of his patients and their friends by carrying this responsibility himself, saying nothing to anybody, yet this very consideration may lead to future embarrassment and subject him to unmerited criticism. For this reason it is better in many cases to state the facts and probabilities frankly, but kindly, to some one in the presence of witnesses, all to be a matter of record. Failing to do this may lead to the suspicion later, that the physician "did not understand the case," and hence was not justified in taking charge of the patient.

It goes without saying that many cases are best examined under an anesthetic, but anesthetics should never be given to women, except in the presence of one or more of their own sex. The records of the X-ray should rest in the hands of the family physician or his consultant, rather than be brought into the case from the outside. The evidence of the skiagram is not always consistent with clinical facts, as regards the function of the structures. This is especially noticeable in certain fractures.

The value of careful records of our cases is in evidence under many different conditions. Surgical notes are of especial importance, as these cases are more often the basis

of malpractice suits, than are any other. For example, should the defendant die before a suit is brought to trial, his heirs would be greatly handicapped in their defense by the lack of full notes of the case. Should the plaintiff die before the trial, the defendant would be unable to testify, unless the executor or administrator chose to do so, and he seldom does. Hence the importance of complete notes as to dates, events, names of consultants, nurses, assistants, visitors, etc.

Furthermore, the time in which suits may be brought is so long, six to twenty years, that the defendant may have forgotten many important details necessary to his successful defence. Notes made at the time of the occurrence of the events will have more weight with everybody, than if made from memory sometime after the events. Under certain conditions complete notes might prevent legal proceedings, and in many other conditions they may be of considerable importance.

In the event of a suit, or of a threatened suit, the defendant should neither talk nor write letters relating to the case in question, as anything he may say or write may be used against him in court. Employ counsel, place the matter in his hands and let him do the talking and writing, as he is protected by his position from prejudicing his client's interests. Have no communication with the plaintiff, except with or through the counsel. It is the business of the attorney to manage these affairs, and he should not be hampered unnecessarily by his employer.

An important function of the state medical societies of the present day is to furnish counsel to their members in malpractice suits without expense. Seventeen state societies have medical defence laws in operation at the present time, and the number is constantly increasing. They are as follows:— New York, New Jersey, Pennsylvania, Maryland, Massachusetts, Michigan, Minnesota, Missouri, Wisconsin,

Iowa, Illinois, Indiana, Ohio, Kentucky, Nebraska, California, and West Virginia. The law is a valuable asset of the state societies, as is indicated by the following extract from the report of the counsel of the Medical Society of the State of New York for 1910:—

"Sister societies of various states are becoming imbued with the determined fight made against blackmail here, and the medical profession is fast coming to realize how important it is that its members throughout the United States should stand shoulder to shoulder in repelling attacks, than which there is no litigation more vicious and far-reaching in its importance, not only in its effects upon the profession, but also in its indirect influence upon the public health."

The experience of the medical profession in New York with this defense law is interesting. In 1899 Mr. James Taylor Lewis, their most efficient counsel, organized the medical defense work for the state society. During the twelve years of his service, over 275 cases of claims for damages have come before him for consideration. One hundred and forty-seven of these cases have been tried and thus far not a single one has been lost and only one lies on his appeal. Not a dollar has been paid by the defendant in a case defended by the society's counsel!

Aside from the financial aid rendered by the medical defense acts, the moral effect upon pettifogging lawyers and their litigious clients goes far to discourage and diminish the number of bogus claims. In several instances a suit has been dropped when it was found that a state organization was the defendant. Fraudulent claims in New York have diminished nearly one half since the inauguration of this law. Five years ago thirty cases were brought in one year, while last year only seventeen were begun. "Were insurance indemnity policies eliminated from the field, even a greater reduction would have been shown."

Mr. Lewis calls attention to another fact that is suggest-

ive, namely, that an increasing number of claims are being brought in that state by women. His explanation is that having become discouraged at their lack of success in fracture suits, the attorneys are resorting to these tactics, hoping thereby to secure verdicts by appealing to the sympathies of the jury in favor of women.

Aside from the defense acts of the medical societies, the increasing frequency of malpractice suits has given rise to two forms of insurance contracts for the protection of physicians. First, a contract to furnish counsel for their defense to a specified limit, and second, in addition to furnishing counsel, to pay damages as per agreement.

Both plans have features commending them to our favorable consideration. The first one simply furnishes counsel, as does the medical society, but does not compromise or settle claims, this resting, as it should, with the defendant. The expense is moderate, and properly managed the plan is a good one.

Theoretically the second plan, which not only supplies counsel but also pays damages, would seem to encourage claims, as in many instances it would be cheaper for the company to settle than to contest them in court. Enterprising attorneys would be prompt to recognize these conditions, and realizing the financial ability of the company, would take every possible advantage of the situation perhaps to the ultimate detriment of the public and of the profession.

Defense conducted by an organization of any sort as mentioned above, is far more likely to be successful than in the hands of individuals. Organizations have no fear of publicity, or of injury resulting therefrom. They have little sentiment, and are hence less tempting to litigants and blackmailers, than are individuals.

A bill was introduced in the Maine Legislature this year prohibiting physicians from insuring themselves against damage suits for malpractice! Fortunately for justice and

common sense, it failed of enactment. Had it become a law, however, there can be little doubt that the Supreme Court would have declared it unconstitutional at an early date.

In conclusion, attention may be directed to the following facts:—Reputable physicians recognize their duty and their responsibility to the public. They have prepared themselves by hard work and the expenditure of time and money to fulfil those duties in a reasonable manner. Their fitness to practice their profession has been certified by the state. They are ready at all times to respond to demands for their services regardless of compensation. Their services are for the poor and the rich. No one, whatever may be his station in life, need suffer for lack of efficient medical and surgical attention. In fact the poor in our cities receive more constant and skilful attention in our free hospitals than the well-to-do class can often command in their comfortable homes. From the moment the laborer is disabled by illness or injury until he is able to resume work, he and his family are under the watchful care of our numerous charities with no expense to himself. Free hospitals, dispensaries, sanatoria, and homes for the needy abound on every hand. Aside from the reputation and experience, the physicians who attend these charities, making their existence possible, receive no compensation whatever for their services. No profession does so much gratuitous work for the public as does the medical profession, amounting to millions of dollars every year in this state alone. This service is rendered willingly and gladly to the worthy poor. The profession insists, however, that in view of the enormous amount of charitable work done by its members in our hospitals and elsewhere, the well-to-do classes shall be required to pay for professional services wherever rendered. This is the only "square deal" possible, and it should be the rule of action in every community. The prevailing abuse of

medical charity is wrong. It is unjust alike to those who support the charities by supplying the necessary funds, as well as to those who render gratuitous services to the various institutions. Medical charity should be conducted upon ordinary business principles, rather than in a manner to encourage dependence and pauperism.

The public having a clear understanding of the facts mentioned in this paper, cannot in justice and reason blame the members of the medical profession for their determination to stand together in opposing and contesting unjust claims brought against them for alleged malpractice, claims brought more frequently, perhaps, by their charity patients, than by others; for rejecting all overtures looking to a settlement of these claims out of court; for resorting to justifiable measures, as medical defense organizations, etc., to protect themselves against imposition and blackmailing schemes for extorting money; for declaring that an unjust claim against a respectable physician lies against not an individual, but an association of hundreds, or even thousands of individuals, whose policy and whose practice it is to contest all claims of this sort to the last extremity, rather than compromise or settle them out of court. Let the public understand that reputable physicians are a unit in this matter, that they will stand by each other in their defense of the right as against the wrong, regardless of time, trouble, and expense; that we mean to do our best for the welfare of our patients, and having done that, we naturally resent being called upon to defend such action at law. Let the people fully comprehend the above facts and conditions, and these unjust and uncalled for suits for alleged malpractice should disappear from the calenders of our courts.

ARTICLE IV.

COEXISTENT EPIDEMIC CEREBROSPINAL MENINGITIS AND MALARIA.

By WILLIAM J. COLLINS, M.D.
OF NORTHAMPTON.

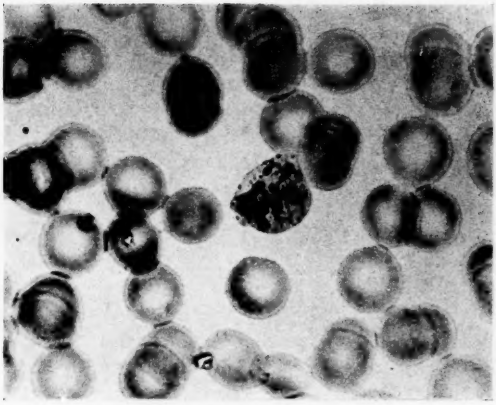
READ JUNE 14, 1911.

THIS is a brief report of three cases of epidemic cerebrospinal meningitis, one tertian malaria and one cerebrospinal meningitis coexisting with tertian malaria. The five cases came from the same locality in Lamia, Greece. This city of 12,000 inhabitants is situated north and west of Athens near historic Thermopylæ. One of the cerebrospinal meningitis cases was first seen post mortem and one eleven days after the onset of head symptoms. The post mortem findings of these two cases are here for your inspection. The others have recovered. The malarial organisms are somewhat unusual, as you will see from the lantern slides and micro-photographs.

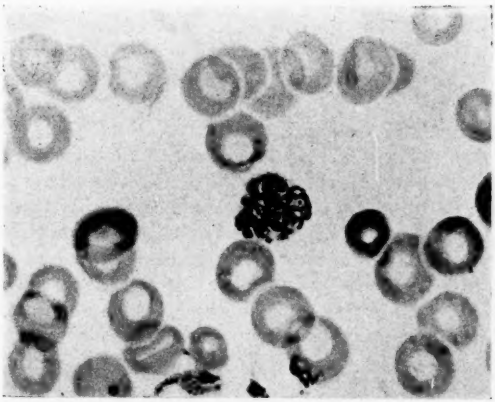
Case 1. G. P. Male, age twenty-four years, was first seen March 4, 1911, eleven days after the onset of head symptoms. On February 21st, three days prior to landing in New York, he had complained of headache but on arrival was able to go to Haydenville, Mass., four hours from New York. On reaching there he had taken to his bed and it was thought that he was "land sick" and would soon be well. Many valuable days were lost, and when he first came under my care the disease was far advanced. There was marked retraction of the head, he rested on his side in a crouching position, knees drawn up and arms flexed. There was herpes about his nose, double Kernig's signs, marked leucocytosis, muttering delirium. Attempts to move his head caused him to cry out with pain. Tempera-

ture 104°, pulse 120, respirations 28, urine and feces not remarkable. On lumbar puncture about 90 c.c. of a serosanguineous fluid was obtained, which contained the diplococcus intracellularis meningitidis. A corresponding amount of anti-meningitis serum was injected, and a slight improvement was noticed. Repeated attempts to obtain fluid or inject serum by the lumbar route were fruitless and it was thought that matting together of the parts at the base of the brain had closed the connection between the ventricles and the spinal subarachnoid space. This was confirmed at autopsy. He became more delirious, was restrained with much difficulty, refused nourishment and at occasional conscious intervals complained of intense frontal headache. His head was drawn so far back that the occiput was between the shoulder blades. He remained in this state until he passed into coma from which he never roused and death occurred seventy-one hours after he was first seen. At autopsy there was an abundant fibrino-purulent exudate at the edges of the brain extending from the optic commissure back over the crura, pons and medulla. Also a purulent exudate on the convexity on either side of the Rolandic fissure. There was much free pus at the base of the brain, as well as a matting together of the parts by a fibrino-purulent exudate.

Case 2. T. G., female, age six years, was first seen March 5th, the day following the diagnosis of case one. She had marked retraction of the head, double Kernig's signs, herpes about the nose and under the chin. Temperature 105°, pulse 124, respirations 26. On lumbar puncture about 30 c.c. of turbid fluid was obtained and the meningococcus was found. On doing a differential blood count the malarial parasite was discovered, and on going over the abdomen carefully the spleen was felt greatly enlarged, reaching within a centimetre of the umbilicus. She was given the serum and large doses of quinine and responded very quickly. The quinine was kept up in her case for two months. The blood was free from malarial parasites and she was allowed to go home with instruction to continue the quinine treatment. Two weeks later she was back with a temperature of 106° and a return of the malaria. The blood showed very marked infection with the organisms, many fields



MULTIPLE INFECTION (5 in one cell).



SCHIZOGONY (Cell Division)

showing multiple infection; one cell having five distinct signet ring parasites, segmentation, free merozoites attacking the red cells and all stages of the cycle. This young lady, coming from classic Greece to this barbaric land, not only had the above named lower forms of bacteria (meningococcus and malaria parasite) but also harbored more highly complex forms of animal life, the pediculosis capitis and the ascaris lumbricoides (round worm), the latter vomited on her second visit to the hospital. This case is still under observation.

Case 3. P. G. Age twenty-seven years, mother of cases 2 and 4 and a neighbor and shipmate of case 1, was first seen post mortem and I am indebted to Dr. Wheeler for the history. She had headache and dizziness about three days before landing in New York (Feb. 24, 1911). She kept about her household duties, however, and could not be persuaded to have a doctor. She thought her headache was the result of her three weeks ocean trip. On March fifth, or about eleven days after her first head symptoms, she became alarmingly sick, temperature 104°, pulse 120, vomiting and diarrhea, such severe frontal headache that she repeatedly yelled, "help my head," convulsions, coma and death two hours after calling a physician. Post mortem: — A thin yellowish exudate was found chiefly along the course of the vessels of the brain. There was a marked injection of the vessels in places, especially over the parietal lobe.

Case 4. C. G. Male, age two and one half years, was examined on the day of his mother's death and the temperature was 105.6°, pulse 126, respirations 30. He had a slight cough, cried much, and while well nourished had an anæmic, pasty color. With the diagnosis of cerebrospinal meningitis definitely established in his mother, sister and shipmate G———, a lumbar puncture was immediately done and a clear fluid obtained. The serum was given at once, as it seemed wise, in view of the circumstances, not to delay even for the bacterial examination. Later the fluid was found sterile and no growth was obtained. On going over the abdomen the spleen was felt greatly enlarged, reaching to the umbilicus. A blood examination gave

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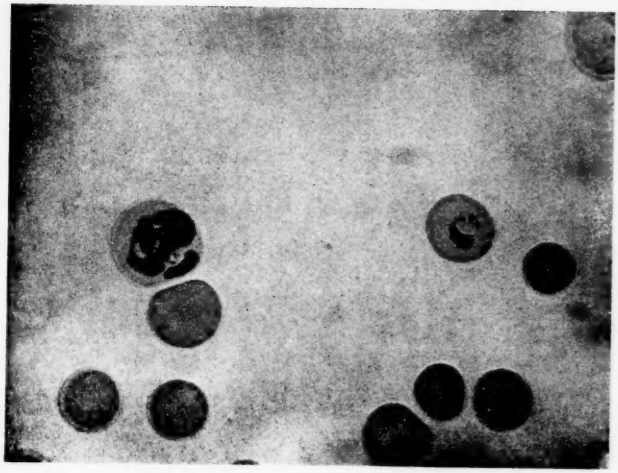
Hgb.50%, polymorphonuclears 66%, large lymphocytes 18%, small lymphocytes 16%, and a very fine specimen of the malaria parasite was found.

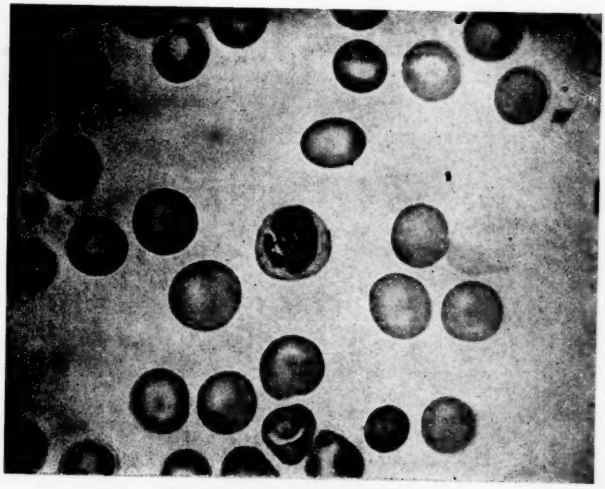
As these specimens show all stages of the cycle and multiple infection, it may not be amiss to describe briefly what you may see under the microscope and on the screen. The name Malaria (bad air) is erroneous, and the idea that the cause was a plasmodium is not now held. The conclusive proof that the cause is a protozoon of which there are several species is now established. They belong to the hæmosporidia (a sub-class of the Sporozoa).

Hæmamœba is the term now used to describe this blood parasite which infects the red corpuscles of mammals, reptiles and birds. Some fields that I shall show you have all stages of the cycle, the free merozoites that is the youngest and smallest forms resulting from segmentation of the adult amœbula or Schizont, attacking the cells, on the cells when they are known as amœbulæ or trophozoites. The different shaped and growing amœbulæ up to adult forms. The Schizonts—In the process of Shizogony the nuclear outline becoming lost and the chromatine becoming divided into a number of small granules which are scattered through the protoplasm; the latter then undergoing corresponding segmentation and the small merozoites result.

The merozoites are of round or oval shape and are set free by rupture of the envelope of the red cells. The onset of the febrile attack corresponds with the stage of Shizogony and the setting free of the merozoites, *i.e.* the production of a fresh brood of parasites. Whether the fever is due to a toxic process or to general disturbance of metabolism is still a question. In this case large doses of quinine were at once given and continued for one month, and the blood examined during this time and no parasites found. On omitting the treatment for a week a chill, vomiting, temperature 106°, enlarged spleen and all the malarial symp-



A YOUNG AND AN OLD PARASITE.



AN ADULT ORGANISM.

toms returned. The blood was again markedly infected. One cell that will be shown contains five definite ring bodies. It is plainly evident that the parasites take to the internal organs, and not finding them in the blood is not absolute proof that they are out of the system.

Case 5. Basil T. Male, age twenty-six years, arrived in New York on steamship Themistocles April 4, 1911, and was first seen five days later. There was a history of five days illness, and at this time he was desperately sick. He had marked delirium, persistent vomiting, involuntary urine and feces, double Kernig's signs, marked leucocytosis, stiffness of neck, cried with pain when head or neck was moved. There was dilatation of pupils when the pain was increased. Temperature 104.6°, pulse 128, respirations 30. He was restrained with difficulty. Lumbar puncture was done and 50 c.c. of turbid fluid which contained the diplococcus intracellularis meningitidis was obtained. The fluid contained a large pus and fibrin clot. An equal amount of anti-meningitis serum was injected. There was a slight improvement in from eight to ten hours, and on the following day less fluid was obtained, but 50 c.c. more of the serum was injected. In twelve to eighteen hours his improvement was very evident and he went on to an uninterrupted recovery. Careful physical examination revealed nothing else remarkable. Kernig's signs were present from eight to ten days.

All the cerebrospinal meningitis cases were isolated. Urotropin was given on account of its disinfecting property and the fact that it is excreted in the cerebro-spinal fluid. Sterilization and fumigation were carefully carried out. Gowns and masks were worn and the cases were kept under observation for two to three weeks after active symptoms had subsided. There was no spread of the disease.

Thoughts suggested by the study of these cases:

1. The great good of the serum if given early and repeated. Thirty cubic centimetres was the minimum dose, and as much more as there was fluid withdrawn.

2. From the pathological findings in case 1, I believe intra-ventricular injection would have been the proper procedure.

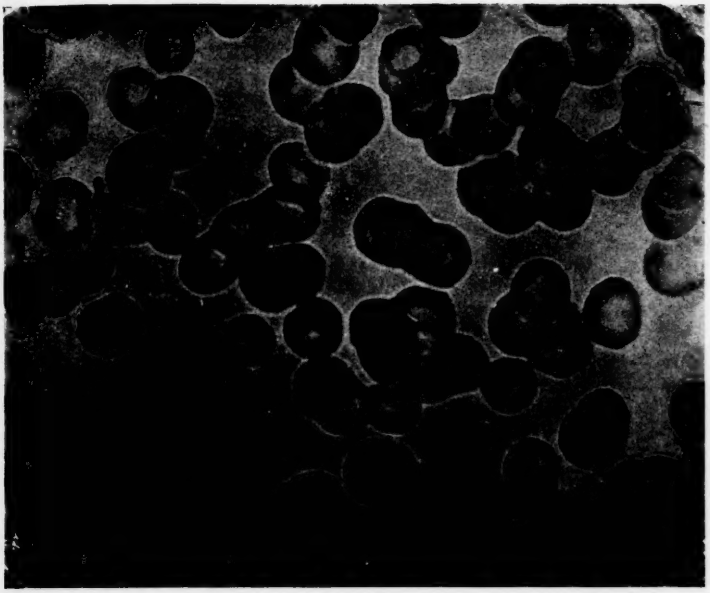
3. That the incubation stage of cerebrospinal meningitis may be very long. These cases came on different ships from the same locality in Greece where the disease was. Reliable information, obtained by correspondence with this city, states that over 100 cases of cerebrospinal meningitis occurred there between August and Feb. 1, the date of departure of the cases reported in this paper. In all the cases the average time from their departure to the onset of active symptoms was four to five weeks.

4. Importance of long-continued administration of quinine in malaria. Cases 2 and 4 each receiving quinine for over a month; the blood found free from the parasite, but on omitting treatment a return of all malarial symptoms resulted. The organisms may be in the internal organs when they cannot be found in the blood.

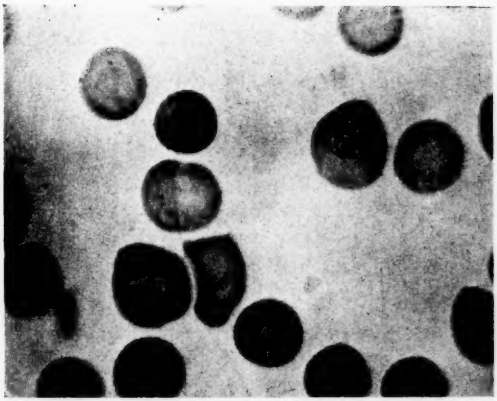
5. The necessity of a careful physical examination as a routine in all cases; one definitely established diagnosis not precluding another.

Example. Case 2, where failure to do either blood or spinal fluid examination one would have been completely at sea. In the case of Costor the child, case 4, with temperature 106°, and mother, sister and shipmate G———, with diagnosis of cerebrospinal meningitis definitely established; on lumbar puncture a sterile fluid was found, but the blood examination at once cleared the situation. This so clearly emphasizes the fact that laboratory methods combined with "hands on the patient" is the ideal combination in diagnosis. In the smaller centers there is too little of this combination.

6. Post mortem work in one's own private practice. The writer has been able to get 90% of post mortems in the last few years. A frank statement of fact in regard to the search for the truth, the gain in knowledge, and the improvements



A SINGLE AND DOUBLE SIGNET, WITH THREE ADULTS.



MULTIPLE INFECTION (4 in one cell) AND ONE SIGNET.

in treatment and diagnosis to result are usually convincing arguments in obtaining permission for an autopsy. This is a much neglected part of our medical work and I am convinced that the fault lies more with us than the laity. An encouraging mark of progress is the fact that three of the last six post mortems were requested by the people themselves.

ARTICLE V.

THE MANAGEMENT OF SPINAL CURVES.

By E. H. BRADFORD, M.D.
OF BOSTON.

READ JUNE 14, 1911.

THE importance of this subject is not generally recognized, but that it should be is evident if the prevalence of spinal curves is considered. A reliable estimate based on careful foreign statistics indicates that at least 10% of our school children are afflicted with some form of spinal curves which can be benefited by treatment, and 3% present spinal curves which urgently need treatment.

Although this is true, the medical profession in general has paid little attention to the subject, largely because the literature of recent investigations on this subject has been addressed more to the specialist than to the general practitioner.

The management of these cases however falls primarily to the family physician, and it is important that he should be well informed in regard to the different methods and the principles of treatment as growing bone subjected to abnormal pressure grows abnormally, and the spinal column during growing years held constantly out of line shapes itself wrongly. The treatment of spinal curves consists in the checking of abnormal pressure and strain on the vertebræ by limiting or preventing faulty position of the column. Where distortions have already developed, treatment should be directed to counteracting them by opposing pressure.

It is manifest that certain bone structures are less resistant to abnormal pressure than others, and more easily shaped in a faulty manner, therefore a part of the treatment must consider the proper nourishment of the growing child's bone, and this includes the hygiene of the child. Hence the treatment of cases of spinal curves interests not only the specialist, but also especially the family physician, and the educator.

As the cases vary greatly in the part of the spine distorted, and the direction of the distortion (*i. e.*, dorsal, lumbar, double, single, forward, backward, lateral accompanied by slight, or by severe twists), and also in the extent of the deformities, varying degrees of rigor of treatment are needed. Hence a classification of the curves will be of convenience to the practitioner. Naturally the simpler the classification the better. The following will serve for ordinary clinical purposes. Those characterized by the existence of

1. A flexibly curved back.
2. A fixed curved back.
3. A markedly twisted back, with evident bone change.

The first group presents slight or no structural changes, but constantly assumed faulty attitudes. (1) From a lack of muscular tone; (2) From faulty habits; (3) From constricting clothing, and (4) From occupations in standing and sitting.

The best method to counteract curves of this class consists in enforcing a daily setting-up drill. What this drill is to be, will depend to a certain extent upon the circumstances of the case. The child should be taught to assume naturally, and easily, without strain or rigidity, the carriage and poise which is known as the normal position, *i. e.* the position of best poise. This should be continued until this gradually becomes the carriage habitually assumed.

The setting-up drill used in drilling recruits may be considered as the standard type of the necessary drill; but it

is evident that in practice, it will be frequently necessary to depart from the rigidity of a drill sergeant and not only vary the exercises, but to give them the character of play, as far as it is possible, and an almost endless variety of exercises can be given which will, if persisted in, develop normal poise. The surgeon should have at his disposal a number of these which may be changed from month to month. In many of the light cases the patients can be left to the care of physical culture teachers, but class work is to be avoided, as individual attention is needed and in many detailed direction is required demanding the training of a physician.

Cases with evident muscular weakness evidently need more than measures which teach patients poise. Certain groups of muscles should be strengthened. These groups are ordinarily the following:

In antero-posterior curves the erector spinæ, the trapezius, the abdominal muscles. In side curves, the gluteii, and the thigh muscles, and indirectly weakness of the leg and feet muscles may be contributory causes of faulty positions which develop lateral curves. For accurate strength measurements tests by a dynamometer and a comparison with standard strengths at different ages and different body weights are necessary; but for practical purposes an inspection and palpation of the different muscular groups at rest and in action will suffice to give an estimate of the region in greatest need of muscle development.

A distinction should be made between exercises which are needed to make correct rather than faulty attitudes habitual, and those which are needed to strengthen certain groups of muscles evidently below par.

The first is evidently the simpler task and can be accomplished by obliging the patient to assume a correct position and stand under the eye of an observer and to make various movements, such as walking, and sitting with the trunk

Exercising Frame.

Patient on correcting exercising frame for lateral and diagonal correction.

Patient seated in frame for antero-posterior correction.

held in a proper position. If this is enforced as a drill two or three times daily, as good results can be accomplished in proportion to the care and persistence exercised. Balancing a book or light basket upon the head may be made to serve as a means of checking faulty attitude.

A simple means of developing groups of muscles is furnished by the careful use of dumb-bell exercises. If these exercises are taken with the patients recumbent, lying upon the back, or the belly, a ready means of individualizing muscular groups is given, if it is remembered that the extended arm, weighted or not, can be used to develop the trapezius group, latissimus dorsi, the pectorals and upper trunk muscles, and that the extended legs, in a similar way, can be used to strengthen the glutei, and abdominal and lower trunk group. Weights and pulleys, rubber exercising bands, bars and heavy dumb-bells can all be employed by patients both in a recumbent and erect position.

It is hardly necessary to state that in treating cases of this class means should be adopted to check the habit of assuming faulty attitudes. For this, it is important to see that patients are freed from clothing which constricts the trunk, checking the normal development of muscles, and from shoes that cripple the feet. It can not be expected that persons can stand normally if the front of the foot is hobbled, or if their heels are supported on the narrow base of a tapered and high shoe heel. Not only should attention be paid to school desks and chairs, but at home in study, in reading, at music practice, provision should be made for such furniture as will not oblige a child seated for any time to resort to a faulty attitude as the one of greatest comfort. The matter is one which demands some attention on the part of parents, who usually furnish for their children at their home tasks the ordinary family chairs which are shaped for adult use.

Where the day's home time of children is spent chiefly

at play, with a constant change of position, there is no need of chairs especially suited to the weak backed; but if the home tasks require sitting for more than half an hour's time, suitable seats are needed. An inexpensive method of furnishing this is by attaching to an ordinary stool of the requisite proper height a triangular shaped seat, furnished with a back similar to that used by stenographers. If the chair seat is narrow there is less disposition on the part of the sitter to sit sideways. The sitter will not slip forward and sit on the end of his spine if the seat is not deep. An adjustable back can be made by furnishing a shaped wooden block six inches wide and convex on its up and down line, adapted to fit in to the hollow of the sitter's back. This block should be made adjustable, but fixed at any point on two steel uprights fastened to the chair seat. An adjustable book rest, such as is purchasable in department stores, fastened to the seat, secures a proper position for the patient when reading.

In especially weak backed children it is advisable to enforce a mid-day rest upon the back, or on the face, of from fifteen minutes to half an hour, upon a hard surface. A mattress placed upon the floor, or a board covered by a blanket, will serve the purpose of a rest to back muscles unequal to the day's work. The position of rest in bed can be disregarded in ordinary cases of this class, and it is rarely necessary to enforce, by the use of a bed frame, the straight position.

When patients with flexible curves constantly assume faulty attitudes it is often unsafe or irrational to rely exclusively on daily exercises which can occupy but a short period of the day to correct the constantly assumed crooked positions. But as the ordinary trade-made back and shoulder braces are either useless or injurious, reliance is usually placed chiefly upon gymnastic exercises to overcome defective conditions and bad attitudes.

Correcting exercise for lumbar region.

Soutter's apparatus for the application of correction plaster jackets.

The same with the frame inclined backwards.

As in growing bone it is necessary to prevent the constant assumption of faulty attitudes in order to check the consequent faulty shaping of the vertebral substances and adaptive shortening and stretching of the adjacent tissues, some form of check is necessary when the habit of crooked attitudes is established. Daily exercise in correct standing is beneficial, but it is impossible to expect a tired child, with weak muscles and undeveloped persistency of purpose, to maintain unaided by force of will the corrected attitude during the greater part of the active day. A simple crutch under the dropping shoulder, slung from the higher shoulder, with slight pressure over the projecting hip or shoulder blade, may be made to serve as a check rein to a horse's head, and can be worn without discomfort during a portion of the day. Straps can be adjusted, connected with a waist band, and secured to the stockings by means of side elastics, and made to pull in the desired direction without interfering with freedom of action and muscle development. Some ingenuity is needed to arrange these satisfactorily, and they must be especially adapted to each case.

Where the curves of the spinal column are flexible the treatment is naturally different from that which is demanded where the curvatures are resistant. The rigidity varies considerably. It may be corrected by manual pressure, or there may be sufficient resistance to make manual correction impossible. In either case less reliance can be placed upon correction by means of ordinary gymnastic exercises. Manifestly, if the patient is unable to straighten the spine, any exercise he is able to make is not corrective, though he may thereby strengthen the muscles needed in the movement. Under these circumstances the surgeon usually directs treatment disregarding the existence of the curve, without attempting to correct it, which is in fact a confession of defeat. But in many instances in growing children, correction of the curve is possible following persistent effort. If the back

of a trained contortionist is examined it will be seen that the normal flexibility of the spine has been increased, and in fact that the spinal column has been made flexible in portions which are normally stiff.

The same principles of treatment are necessary where an abnormal stiffness is present. But ordinary free exercises are not sufficient, for these simply increase the flexibility of the column in the regions where flexibility is present and not in the curves where abnormal rigidity persists.

Where the condition of resistant curves exists, and is due to a shortening of the ligaments, fasciæ, and muscular attachments, and not to an alteration in the shape of the bones, the surgeon can attempt treatment with a reasonable expectation of improvement if corrective measures are followed with persistence.

It has been demonstrated by experiments upon cadaver, that if pressure is made upon the ribs near their attachment to the spine and in the region where they curve around the side, and if a force is applied in an opposite direction to the column above and below this, the spinal column will be curved at or near the point of pressure. It is therefore needed that in correcting exercises designed to stretch shortened ligaments aud fasciæ, the abnormally resistant portions of the spine be determined, which can be done by a careful inspection of the spinal column moved in various directions, and that pressure and counter-pressure be constantly exerted at the desired points, and that all correcting exercises or movements be conducted while such pressure is made. For this purpose some form of mechanical arrangement is needed, and many mechanical devices have been furnished. For the ordinary use the simplest are manifestly preferable.

A serviceable form of arrangement for exerting pressure, which causes the patient but little discomfort, can be furnished in the following way. A quadrangular iron pipe

Cadaver Demonstration showing that side pressure on the ribs will curve the spinal column opposite the point of pressure.

Recumbent frame with lateral correction

frame is made as long as the patient's full length, slightly wider than the trunk and higher than the length of the thighs. Cloth straps are placed across this at appropriate places to exert pressure upon the pelvis, the upper part of the chest and the head of the patient, who kneels, placing the head, face downward, and trunk upon the above mentioned straps across the frame.

Correcting side pressure can be exerted at any portion of the trunk by hanging weights over the side of the frame. These weights are fastened to straps passing around the patient's neck, trunk, and hips at any desired points, or by means of rod levers, which can be made to press as may be necessary. The patient can remain in this position without great discomfort, subjected for an hour or so daily to any desired degree of correcting pressure.

The condition is different where in addition to faulty attitudes due to weakened muscles, and in addition due to shortened ligaments, fasciæ, aponeuroses, and muscles, distortion in the shape of the bone and abnormality in the relative position of the different vertebræ and of the ribs are present, and when a twisted or distorted spine is to be considered.

The success with which this condition can be treated will naturally depend upon the amount of the distortion and the resistance of the bone. Under certain circumstances, in bad distortion and in strong children and adults, correction of the distortion is impossible, and it was formerly thought that this is true in all cases of markedly distorted bone, but recent investigations, confirmed by practical experience, have shown that growing bone in the spinal column is capable of being shaped under pressure, as has been shown in dental surgery to be true of the jaw, and of the skull among flat headed Indians.

This is accomplished by applying a constant direct pressure in the regions of the trunk where distortion exists. Success has frequently not been attained in the treatment of

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curves of this class, owing to the difficulty of applying the correcting force directly upon the distorted parts.

When it is necessary to exert a constantly efficient correcting pressure upon a distorted and twisted spine, the most effective method at the surgeon's disposal is by the application of a non-removable plaster jacket. To be thoroughly efficient, however, a plaster jacket needs to be applied with much care. In all curves above the shoulder the jacket, in order to secure an adequate point of counter pressure, should not end at the axilla, but should extend over the shoulder and around the neck. If this is not done, the upper edge of the jacket under the axilla exerts a pressure on the concavity of the curve, as in time the jacket shifts around the trunk, and although this may be corrective at one part of the distorted trunk there is an increase of deformity at another.

Windows should be cut out of the jacket freely over the concavities of the distorted trunk, in order that the patient in breathing should expand in the hollow part and be compressed in the projecting portions of the distorted thorax. Surgeons are frequently deceived, by the twisting of the plaster jacket, into the belief that a marked degree of correction has been rapidly gained, and the cast becomes more loose. Gain in correction is necessarily gradual, and if a marked change takes place rapidly, it should be taken as an evidence of some displacement of the corset.

In order to increase the effectiveness of the pressure, the following device can be employed. The ordinary hot water rubber bag can be furnished with a short rubber tube screwed at one end to the stopper of the bag, and at the other furnished with a tip which can be screwed into the end of a bicycle pump. If the rubber bag is placed over the portions of the trunk requiring pressure, and the jacket applied so that the tube attached to the bag protrudes through the plaster, by inflating the bag increased pressure can be furnished.

The position in which the patient is placed during the application of the jacket varies with the curve. It can be applied with the patient suspended vertically; when the patient is seated; with the patient lying face downward, or upon the side, with the trunk supported on straps stretched over a frame; or with the patient kneeling face downward, the trunk supported in the same way. In the prone position the superimposed weight of the upper part of the body is eliminated, and it is easy to apply correcting pressure while the bandages are wound around the patient's trunk. In the seated or the kneeling position the exaggerated lumbar curve, the hollow back, is eliminated, and the round shoulders and dropping forward of the head are best corrected by vertical suspension.

Some form of correction during the application of the jacket is advisable. This can be furnished in various ways; by adjusting straps connected with a frame, in the middle of which the patient is suspended or placed; by arranging pressure rods and plates, or by manual pressure.

Plaster jackets should be applied with much care, for they may become clumsy, heavy instruments of torture if proper consideration of the comfort of the patient is not entertained. If the jackets are not too heavy, and if windows are cut out at suitable places, they can be made in such a way as to give but little discomfort. They should be removed every month or two.

The limit of time for the usefulness of plaster jacket treatment is less than was formerly supposed. Plaster jackets can be made to correct bone distortion, but they are injurious to the muscular tissue, causing muscular atrophy. They should be worn for as short a time as possible and only employed when milder measures have failed. Splitting a jacket and making it removable diminishes the effectiveness for correction without removing its injurious qualities in weakening the back.

The problem of furnishing an adequate and non-injurious correcting pressure to patients needing this, in place of plaster jackets, or after discarding jackets, is not an easy one. It is manifestly absurd to apply a jacket for a few months, remove it, and allow the patient with weakened structures to slump back to the former distorted position, and to continue the use of a jacket, constricting and weakening the trunk during the growing years, is harmful. Removing the jacket and allowing the patient to take gymnastics for a short time each day only partially meets the difficulty.

What is necessary is a light appliance which does not constrict, which is not disfiguring, which can be worn under the clothing,—adjustable by the patient or the patient's mother,—which is not expensive, and which allows as much free motion as is compatible with efficient correcting pressure.

While the ideal has not been reached, progress has been made in that direction, and appliances can easily be made which, while not furnishing as affective correction as a plaster jacket, can be made to serve as a substitute for them in the less severe cases, and succeeding the plaster jackets during the growing period when some support is needed.

An appliance of this sort can be made by attaching to a pelvic steel band two steel uprights, one reaching from the neck to the buttock behind, and the other, in front, passing over the projecting portion of the distorted thorax. A strap connecting the upper part of these furnishes counter pressure in the neck; the pelvic band gives another counter point at the hip, and a correcting force can be applied by means of a leather strap passing over the projecting shoulder or projecting ribs. By furnishing a crutch on one side, riding upward of the appliance is prevented, and by bracing the uprights by cross straps the desired stiffness can be given. Each case needs a specially devised appliance, but as the appliance can be made easily and requires little mechanical

Correcting exercise, lateral and diagonal, for the mid-dorsal region.

Front view, correcting brace.

Back view, correcting brace.

Correcting brace with leather half shell, front view.

Correcting brace with leather half shell, back view.

work some thought is necessary on the part of the surgeon in selecting the regions requiring pressure and in planning the making of the appliance.

In directing the treatment of these cases, it is necessary that the surgeon should be able to furnish himself with some means of measurement of the curves, which will mark improvement in the progress of the case, or the reverse. For absolute accuracy the matter presents difficulty, but a practical means of measurement of a serviceable degree of accuracy is afforded by marking upon the back a plumb line reaching from the seventh vertical vertibra to the buttock, and measuring the divergence of the line of the spines of the vertebræ from this vertical line, measuring also the distance of the tips of the scapulæ from the vertical line. The surgeon, in this way, is able to chart the course of treatment in the long period when supervision or active treatment is necessary.

It is to be remembered that the treatment of spinal curves differs from that of other affections in that it must be prolonged, and is never attended by rapid improvement. It is a guidance of growth rather than a treatment in the ordinary sense.

With all the means of combatting and correcting spinal curves in growing children at his command, the practitioner will need especially to exercise sound common sense to guide him in selecting the case suited for special treatment. In some instances no treatment is demanded beyond the regulation of proper hours of rest and play. In others, thorough attention to the details of adjusted correcting pressure, with careful enforcement of ordinances in regard to daily exercises, hours and attitudes of study, if an increase of a threatening curve is to be expected or correction reasonably hoped for.

Frequently the need of a searching consideration of the patient's general condition is overlooked, *i. e.*, the rate of

growth, the relation of height to weight, the digestion, metabolism, temperament in its relation to the wear and tear of daily life. All these are factors in the problem of opposing not a disease but a vicious tendency of growth.

The slow development and long period of supervision in cases of spinal curves are factors of importance in the treatment of these cases. These cases can not be entered on the note book and discharged as relieved, or cured, after a few months' treatment. Inspection at stated intervals during the period of growth, with careful record of the curves and the enforcement of suitable measures at the different stages of progressing changes, are necessary on the part of the practitioner if he wishes to do full justice to the case placed under his charge.

Of great difficulty in the management of these cases is the problem of regulating school work and study. Schools are organized and administered for normal children, and under the necessary regulations for the enforcement of habits of regular work it is difficult to arrange for elasticity in the curriculum suited to cases which, though not invalids or sick, are not capable of the strain which the average child can bear with little detriment.

If every child at school were given each day a load to carry, and that load was made heavier, not according to the child's strength, but according to age or ability to pass an intellectual test, the absurdity of such a plan would be manifest; but in fact the assignment of school tasks, without a clear consideration of the child's physical condition, is almost equally irrational. A too rapidly growing over-grown child is frequently dull and should not be pushed; but even if not intellectually dull such children—the under-nourished, the anæmic, and under-vitalized—need special school tasks as compared with children more nearly approaching normal standards of condition.

The needs of children of this class are so apparent that in

several cities in Europe special schools are established for children with spinal curves, with school and play hours, and gymnastics especially adapted to the needs of the children.

In Berlin special classes are arranged with tasks adapted to the child's condition. And this plan should receive the careful consideration of our educators, for it is manifestly better to adjust the school child's burden to its strength, rather than to attempt to stretch the child's strength to a fixed burden.

Although the problem is primarily one for educators rather than physicians, the medical profession has a duty in the matter, since, because of its training, it should be better able to detect physical defects and to define practical tests for physical conditions.

It would be unjust to lay the blame of the great prevalence of spinal curves among our school children to our schools, when the home conditions form so important a factor; but the fact that education lays claim to a large portion of the child's actual time places those in charge of education in a large measure in a position of responsibility relative to the guidance of the child's growth and nurture. In fact, the educator, the family physician, and the surgeon should regard themselves as partners in the task of providing measures which counteract the influences, too potent at present, tending to distort the growth of children.

DISCUSSION.

DR. WILLIAMS, Waltham: I would like to ask one single question with regard to a method that has been more or less popular, the suspending of the child by the head where the plaster of paris jacket is used. Has this method been discarded?

DR. BRADFORD: I do not mention it, not because it has been discarded. The suspension method is an excellent

one for the surgeon, but not as comfortable for the patient, and other methods are as effective if not more so in correcting curves.

DR. EMMONS, Boston: I would like to ask Dr. Bradford how soon an infant should be allowed to sit up. Is that an influential factor?

DR. BRADFORD: When a child should be allowed to sit up is a question of judgment. There are infants and infants. A child that has any evidence of rickets should not be allowed to maintain an upright position for any length of time.

DR. TORREY, Andover: Dr. Bradford has stated that the general practitioner does not take special interest in these cases. I would like to call attention to one thing that has troubled me. The men out in the country are in the habit of sending orthopedic cases either to a specialist, if they are able to do so, or to send the cases to the children's hospital, and in sending my cases to specialists, I have always had courtesy and a great deal of helpful suggestions and a good deal of coöperation from the specialist as to what I am to do in regard to following up the case. I have not always found it to be true in the hospital cases. I have found difficulty in knowing how to look after a case after sending it to the hospital.

I think we might have a little more definite instructions when the case comes back, to give to the mother, a little more helpfulness from the hospital authorities. It would be quite a help to those cases which come from a distance.

DR. BRADFORD: What you say is quite true, yet look at it a moment from the hospital side. The treatment of spinal curves is difficult to arrange at a hospital. It is difficult to get enthusiasm among the student assistants and nurses. Their work is very laborious. A better coöperation between hospital authorities and the outside physicians is something that will come, and those in charge of hospitals will endeavor to get it. After all is said the practioner is the one who has to do the bulk of the work in training the parents. The treatment is laborous, and the tax upon assistants and nurses great.

In reply to a question as to the value of massage, Dr. Bradford said, massage is one of the beneficial things, and in small children is very good, but to say it is of great benefit is going a little too far. Massage is beneficial without doubt, but in correcting curves more thorough means are needful.

DR. HOGNER, Boston: I should like to ask the doctor how much value he puts on hanging by the hands.

DR. BRADFORD: It is useful for certain curves, especially where the curve is a lumbar one. On the dorsal curves, which are the hardest ones to treat, the hanging alone, strengthens the weaker portions of the spine, and does not have as much effect on the stiffer portions of the spine.

SYMPOSIUM ON ANESTHESIA.

ARTICLE VI.

SOME CONSIDERATIONS OF ETHER ANESTHESIA.

By DANIEL F. MAHONEY, M.D.,
OF BOSTON.

READ JUNE 13, 1911.

It is not my intention to deal in an elaborate way with anesthetics and their administration, but merely to dwell upon and emphasize certain practical points which impress me with their importance.

The busy practitioner in the city or country frequently meets with patients whose condition demands surgical anesthesia, and whether he is obliged to administer the anasthetic himself, or call upon another to perform this duty, is often a question of circumstances. He cannot be expected to have constantly at hand complicated and expensive apparatus for the induction of anesthesia by various methods, and this is not always necessary. But it is of the utmost importance that he should be familiar with the physiological action, and the untoward effects of at least one anesthetic that he can use in his practice, also at least one simple and safe method that may apply to the majority of his cases.

The secret of successful anesthesia depends chiefly upon the condition of the patient, the method of administration, and the care and attention of the anesthetist. The patient's condition we are frequently obliged to take as we find it,

the method of administration may rest wholly upon our conveniences and skill, but the attention and consideration of the anesthetist should never be a fluctuating element.

To some of you gentlemen it may seem as though I owe you an apology for asking you to consider many of the topics of this paper. Already many of the suggestions may have been incorporated in parts of your routine work; still I have been asked these questions many times by students while they are receiving their practical instruction in anesthesia, and also by practioners who manifested more than a casual amount of interest in the subject.

We have not done all that should be expected of us when we have watched our patients through the condition of surgical anesthesia, but it should be our constant aim to make the induction and post-anesthetic stages as comfortable as possible.

To be brief, let us consider the questions under three different headings, all of which bear an important relation to each other; namely,

Preliminary Preparations.
Technique and Conduct of Administration.
Post-Anesthetic Care.

Preliminary preparations depend upon the exigencies of the case, and we will assume that the doctor can elect his own time for administration. Patients addicted to the use of alcohol invariably do better while under ether if they have had a preliminary treatment with bromides for a week or ten days with a gradual withdrawal of alcohol. Even a few days treatment is better than none at all. This type of patient usually taxes the skill of a trained anesthetist before the operation is completed, and I feel that to those who anesthetize only occasionally, any suggestions that will help them with this class of cases will be welcomed.

Considerable quantities of the anesthetic are employed to keep the patient quiet. It is occasionally difficult, if not

impossible, to secure total muscular relaxation in patients of this class, and reflex movements during operations upon sensitive parts may obstinately persist, even though dangerously large quantities of the anesthetic be administered.

The condition of the heart, lungs and kidneys should never be overlooked. With attention to these salient points before anesthesia you will lessen your anxiety during the patient's early convalescence.

Active purging of patients before operation is to be discouraged. Previous to abdominal operations, a reasonable cleansing of the bowels the day before operation by one of the common salines (Epsom Salts or Seidlitz Powders) or Castor Oil followed by an efficient enema one or two hours before operation is sufficient.

In Goitre cases with pronounced nervous symptoms and tachycardia, I believe we cannot do better than adopt the method of Crile which he refers to as "Stealing upon the Thyroid." It is briefly: about five days before he expects to operate, he tells the patient he will be given the "oxygen" treatment every morning. This consists of a hypodermic injection of sterile water, and inhalation of fresh air alone or with a very little nitrous oxide gas through the inhaler that will be used on the morning when true anesthesia will be induced. This treatment is not carried to the stage of unconsciousness and lasts only about five minutes each day.

A temporary increase of the nervous symptoms takes place after these treatments, but the disturbance is less marked as the number of treatments increase. On the morning of the operation, the patient receives the treatment as usual. Morphine, gr. $\frac{1}{8}$ or $\frac{1}{6}$, is substituted for the daily hypodermic injection of sterile water, and at this time it is carried to the stage of surgical anesthesia with nitrous oxide, then ether. Crile is convinced that by this method he eliminates an important factor in the production of shock; namely, the anticipation of operation, and the excitement attending the induction of anesthesia.

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There is a difference of opinion whether the anesthetic should be started with the patient in bed, or on the operating table. Some believe there is less excitement on the part of the patients if the anesthetic is started while they are in bed, and that is the only reason in favor of this method except in cases of multiple injuries and fractures when the slightest movement causes much pain. When the anesthetic is started with the patient on the table, there is less danger of injury in transferring from bed or truck to the table, and the anesthetist and his assistant can invariably manage the patient better during the excitement stage. If the patient is only lightly anesthetized at the time the transfer to the table is made, the handling is enough of a stimulus to prolong the excitement stage and often induces vomiting. This method has been adopted in the clinic of the Mayo's, by Crile, and also in many of the hospitals in this city.

Whether we shall administer morphine alone, or in combination with atropine before the anesthetic, is a question frequently asked. Labbé and Guyon seem to have been the first to administer morphine before chloroform in surgical practice. They used it with Claude Bernard's idea, namely, that of facilitating the action of chloroform and rendering smaller quantities of the anesthetic necessary. They found by this practice the excitement stage of chloroformization was considerably lessened, and that when once anesthesia had been produced, extremely small doses of the anesthetic were needed to maintain insensibility to pain. The same reason applies when ether is used.

I have used morphine with atropine before giving ether in a large majority of cases of adults, and have been satisfied with the results. When the patient was under fifteen years of age, I have used atropine alone. Morphine, grain $\frac{1}{4}$ or $\frac{1}{6}$, and atropine grain $\frac{1}{100}$ or $\frac{1}{120}$, is the usual adult dose, and atropine grain $\frac{1}{150}$ to grain $\frac{1}{240}$ for children.

This should be given subcutaneously, and always at least twenty minutes before the administration of the anesthetic. Exceptions to the use of these drugs are patients with drug idiosyncrasies, and to the use of morphine in weak subjects and cerebral cases in a semi-comatose condition. Omit atropine in exophthalmic goitre with tachycardia.

The advantages of the use of these drugs are that many cases demand less anesthetic, the excitement stage is shortened or less marked, and it lessens the post-anesthetic nausea and vomiting. Tiersch employed this method in several operations about the mouth, and found it possible to maintain an analgesic state in which the patient, although unable to feel pain, could aid the operator by coughing.

Technique and Conduct of Administration. Before the anesthetic is started, the administrator while explaining to the patient what is expected of him, should speak to him in an encouraging manner, thereby getting his confidence if possible. At the same time he should "size up" his patient, thereby obtaining a good mental picture of how he looks. Ask yourself the question: "Does this patient look healthy and strong, or weak and not very resistant?" I believe we all agree that patient's faces are good indicators of their physical condition. A patient obliged to submit to operation may have been under a severe mental and physical strain for some weeks before, and his face usually shows it. His pulse may be of good quality, still his physical condition may be far from good. If you get a mental picture of the patient's face before anesthesia, you will have something for comparison from time to time. As the operation advances any change in the patient's condition almost invariably shows in his face.

There is some difference of opinion whether ether should be administered by the semi-open method, using the Blake metal cone, or one made of a towel and pasteboard, or by the open, "drop" method. Each has its advocates. The

fact to bear in mind is that the patient should be allowed to have as much air as possible with as much ether as is necessary. The "drop" method has become very popular of late years, and is very satisfactory in many cases. There is, however, a class of cases, namely, very alcoholic and extremely nervous individuals, some athletes and patients of large stature, that are almost impossible to anesthetize by this method, and demand the greatest dexterity on the part of the administrator. With this class of patients I prefer to use a cone.

The principal objection raised against the "cone" method is that the patient does not get sufficient air. This objection may be removed by the proper preparation of the cone and by the frequent renewal of part of the packing, or the use of a fresh cone. When the cone is used, small amounts of ether should be used frequently rather that saturating the cone from time to time. The satisfactory use of the cone might really be called another form of the "drop" method.

Either from abnormal collapsibility of the alæ nasi, or from paralysis of the dilatores narium, in some patients during inspiration the alæ act as valves, being drawn in during every act of inspiration, preventing all access of air; sometimes a deviated septum or hypertrophy of the tissues over the turbinates interferes with the entrance of air, genuine asphyxia resulting from both conditions, puzzling to the anesthetizer because he cannot recognize the cause. An aural speculum in each nostril as suggested by Nancrede, or keeping the teeth and lips slightly open by means of a gag will relieve this form of cyanosis, which is seen not to be due to the tongue falling back, because its tip rests against the front teeth; nor to the base of the tongue obstructing the pharynx, because elevation of the jaw produces no effect upon the cyanosis; and not to mucus, because the noise produced by its presence is absent, and every time the mouth is opened the character of the respiration improves and the cyanosis diminishes.

Instead of using the aural specula in this class of cases, I have frequently used soft rubber tubes. These tubes are about the size of, or slightly smaller than that of an ordinary fountain syringe, and two pieces six inches long. One of the ends of the tubes is cut obliquely across, and through the other ends are inserted at right angles, medium sized safety pins. These tubes are lubricated and introduced into the nostrils along the floor of the nose. They should be introduced slowly and stop when free breathing through the tubes is established. Sometimes it is necessary to introduce the tubes until the safety pins press against the alæ nasi.

Crile uses rubber tubes in a similar manner for operations about the face and mouth. His apparatus is simple and very efficacious, and consists of two soft rubber catheters, size 24 or 26 French scale, with their tips cut across obliquely. The other ends are connected with a Y shaped hard rubber tube, such as that part of a Bowles' stethoscope. A piece of rubber tubing connects the other part of the Y tube with a medium sized glass tunnel.

The patient is anesthetized in the usual manner and the head of the table raised. The lubricated catheters are gently passed along the floor of the nose for about six inches, behind the base of the tongue to within three-fourths of an inch of the epiglottis. When the breathing through the tubes is perfectly free, you know they are sufficiently introduced. Sometimes it is necessary to withdraw the tubes and reinsert them before free breathing is established. A mouth-gag is then introduced, the tongue is drawn forward and the pharynx packed lightly with gauze. A small piece of loose gauze is placed in the tunnel and the ether dropped upon it. This gauze is changed from time to time during the administration.

The advantages of this method are: (1) It prevents inhalation of blood and mucus, (2) It produces an even anesthesia, and (3) The anesthetist is out of the way of the

operator. I have used this method in thirty cases for removal of tonue, tonsil, superior and inferior maxillæ, and cancer of the lip with very satisfactory results.

Post-Anesthetic Care. From the time the administration of the anesthetic is discontinued until the patient has recovered consciousness, he should be closely observed, as it is during this stage that many disturbances of respiration may arise. Too much care and gentleness cannot be exercised in the transfer of the patient from the table to the truck or bed. By placing a folded sheet under the patient while on the table and using it as a hammock, he may be transferred with the least amount of upset, thereby preventing or minimizing the post-anesthetic nausea and vomiting.

It is an excellent practice, when circumstances permit, to turn the patient well upon his right side when he is put back to bed, as suggested by Hewitt, and place a pillow at his back. In this position, stertor generally ceases; the tongue gravitates to the side of the mouth; a free air-way is established; mucus and saliva are not swallowed; and should vomiting occur, any vomited matter will readily find an escape without interfering with breathing. The patient's chin should not be too near the sternum, as it may interfere with breathing and retard the recovery.

It is the opinion of many observers that one of the common causes of post-anesthetic nausea and vomiting is an irritation of the gastric mucous membrane by ether ladened mucus which has been swallowed. This condition may persist and become distressing despite our efforts before and during anesthesia to prevent it as far as possible. Gastric lavage is frequently tried. Some patients will wash their own stomachs without subjecting them to the use of the tube. Hewitt advocates giving the patients a tumblerful of hot water to which has been added a small teaspoonful of bicarbonate of soda. Buxton speaks highly of the addition of bicarbonate of soda to black coffee. The atropine given

before anesthesia certainly helps to overcome or diminish this stomach upset. Neurotic patients are frequently relieved by the use of an enema consisting of sodium and potassium bromide, of each fifteen grains, in four ounces of warm water.

Occasionally we meet with a case in which we have made every effort from start to finish to prevent the post-anesthetic discomfort. Some of these cases obstinately resist all treatment, but with this type of case we must not lose sight of the fact that the persistance may be due to Acetonemia. This condition may be recognized by nausea and vomiting, patient in a drowsy condition, sweetish odor to breath, and the presence of acetone in the urine. Copious enemata of a saturated solution of sodium bicarbonate administered slowly, and teaspoonful doses of Aromatic Spirits of Ammonia given frequently by the mouth may relieve the condition.

The sincere interest and coöperation of the medical profession in regard to general anesthesia during the past few years is certainly very gratifying. The added stimulus this subject should receive from our symposium to-day should do much toward raising it to the high place that its importance warrants.

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SYMPOSIUM ON ANESTHESIA.

ARTICLE VII.

REMARKS ON LOCAL, SPINAL, AND GENERAL ANESTHESIA.

By FREEMAN ALLEN, M.D.,
OF BOSTON.

READ JUNE 13, 1911

Before surgical anesthesia was discovered surgeons were eagerly seeking for some drug which would alleviate the pain of surgical operations. With the discovery of ether and chloroform came drugs which would not only alleviate but totally abolish pain.

This original purpose of anesthetic agents, however, namely, the mere abolition of pain has been entirely lost sight of in the progress of surgery, and especially since the development of abdominal surgery. This was found to require much greater depths of anesthesia than had ever before been attempted, and surgeons demanded that their patients be not only free from pain but absolutely immobile and paralyzed by the anesthetic agent. So it soon became evident that surgical anesthesia was not an unmixed blessing.

Without dwelling upon the suffering and dread experienced by patients while undergoing an unskilled administration of these anesthetics,—post anesthetic nausea and vomiting, pneumonia and nephrites and death from ether and chloroform occurred, and still continue to occur, although much less often in communities where anesthetics are administered by trained anesthetists.

Now it seems to me that the lines along which true pro-

gress in anesthesia is to be made are in a return to this original idea of anesthesia—namely the mere abolition of pain. The suffering of the patient who is going under anesthesia, the nausea, vomiting, anesthetic shock, pneumonia, nephrites, and above all death, must be avoided. The patient must be put through his operative crisis with his resistance as little impaired as possible. Surgeons must learn to put up with minor operative inconveniences to themselves for the sake of their patients.

A most important step to this end is for more young men to adopt the profession of anesthetist and to work at it to the exclusion of other forms of work. The field of anesthesia work is quite large enough to occupy the entire time, and if thoroughly gone into to satisfy the ambition of many men. And surgeons should employ these professional anesthetists. It is not fair to the patients to subject them to the suffering that may be produced by unskilled administration of anesthetics when skilled administration is to be had. I constantly meet with patients, notably women and children, who have become imbued with the profoundest dread of taking anesthetic owing to the stupid and unskilled administrations their surgeons have allowed them to be subjected to.

The following are, in my opinion the best methods known to-day for the purpose already mentioned — namely, the abolition of pain with the least possible danger during, and untoward effects following, anesthesia.

In the first place there are certain operations that can be performed without any anesthetic at all. When this is possible it is of course the very best line of practice. For example, I was asked by a gynecologist to examine a woman as to the best anesthetic for removing a uterine polyp. She had myocarditis, and while it would have been quite possible to administer spinal or general anesthesia, it was agreed that an attempt first be made to remove the polyp

without any anesthetic. This was done without causing the patient the slightest pain, and no anesthetic was necessary.

In the second place, all operations that can be so performed should be done under local anesthesia. This is by far the safest line of anesthetic practise, but one which is much neglected by most surgeons. A visit to the clinic of Prof. Bier of Berlin is a revelation of the perfection to which technic and methods of local anesthesia can be carried. Very many major operations are successfully and painlessly done under local anesthesia. The average surgeon in this country has no idea of it. Without going into details I may say that Bier has entirely discarded Cocain for this work. He uses a one or two per cent. solution of Novocain with adrenalin, which is exactly as effective and much less toxic. As much as 40 c. c. may be injected if necessary without producing any toxic symptoms. Special syringes and needles are also necessary.

Third. A word about morphine with atropine or scopolamine. It is not only justifiable but desirable to more or less thoroughly narcotize a patient who is about to undergo a surgical procedure of any considerable extent. A patient so narcotized is placed in a much better frame of mind, his dread is removed, he is tranquillized and placed more under our control. If general anesthesia is to be employed the amount of the anesthetic agent necessary to control the patient can be diminished by one half in many cases. The addition of atropin to morphine maintains the vaso motor tone, and by preventing the excessive secretion of mucus and saliva is a great safeguard against post anesthesic pneumonia. It is true that ether or chloroform will be eliminated more slowly, but a quieter recovery from anesthesia results, and is much less likely to be attended by nausea and vomiting. In gas and oxygen anesthesia it is imperative to have the patient well narcotized beforehand with morphine and atropin, and in many cases a dose of scopola-

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min helps a great deal. In spinal anesthesia especially if it is to be done before an audience, the same is true.

Fourth, Spinal Anesthesia. This is an exceedingly valuable addition to anesthetic methods. The experimental stage of spinal anesthesia has long since passed by and it is now no longer dangerous. In the days when cocaine was used and adrenalin was not used it was dangerous, and many deaths resulted. But to-day cocaine has been abolished, and it is known that the addition of adrenalin to any substance to be injected into the spinal canal is absolutely essential to safety. Stroain, Norocain and Tropacocaine are the drugs used. Using the latter drug with adrenalin and following strictly the technic laid down by Bier, I consider spinal anesthesia to be an exceedingly safe procedure. Statistically I am unable to give you any useful idea as to its safety. Jonnesco has well over 1,000 cases of cervical and lumbar punctures without a death. Tuffier, of Paris, claims 10,000 cases with no fatality, including his cocain cases. I can only say that I have the utmost confidence in the method, and in my 220 cases have seen only one patient who gave me even the slightest cause for alarm. That patient made a perfect recovery. After effects have been almost entirely absent in my cases. The best subjects for it are old and middle aged men, and the best operations are ærinotomy, prostatectomy, hemorrhoids, fistula in ano, Kraske's operation, supra pubic cystotomy. Among my cases are two of double amputation in railroad injuries, the patients being in profound shock. Both patients had spinal anesthesia and lived. Spinal anesthesia absolutely blocks shock, by preventing entirely the transmission of impulses from the periphery to the great centres. It absolutely spares the heart and lungs, but does not spare the kidneys, as albumen and casts often appear in the urine after spinal anesthesia. A strong indication for spinal anesthesia is diabetes. In my 220 cases I have had fourteen failures,

nine of which were due to anatomical conditions. That is owing either to extreme obesity or to steo-arthritis of the lumbar spine, and I have been unable to make the puncture.

It is contra-indicated in acute infectious diseases, in suppuration near the seat of the puncture, in any disease of the central nervous system, in all fevers and in recent syphilis.

Fifth. Nitrous Oxide and Oxygen anesthesia has lately been exciting a great deal of interest in this and other communities, and as there exists a considerable confusion of opinions as to its safety I should like to tell briefly what I know about it.

To begin with, there is nothing new about this form of anesthesia. It has been practised to a very considerable extent in this country during the last ten years. In 1903 we did about fifty cases at the Mass. General Hospital, using a Hewitt apparatus. It seemed to be a very attractive form of anesthesia, and Dr. Hewitt's testimony as to its safety gave one great confidence in using it. He says the following: Hewitt, Anæsthesia, p. 265 (1901).

"There is no form of anæsthesia at present known which is so devoid of danger as that which results from N_2O when administered with a sufficient percentage of oxygen to prevent all asphyxial complications. I have employed this system up to the present time (Jan. 1901) in about 14,000 cases, as near as I am able to estimate, and with the exception of two or three cases in which, owing to the percentage of oxygen having been insufficient, transient respiratory embarrassment arose, I have had no anxiety. Owing to the fact that little or no strain is thrown upon the circulation, persons with feeble and dilated hearts are anæsthetized by nitrous oxide and oxygen without that slight risk which obtains when the former gas is administered alone. Moreover, those patients who, by reason of senile or other changes in the thoracic parietes, pleuræ, or lungs might evince symptoms of embarrassed breathing at the acme of ordinary nitrous oxide

anæsthesia are certainly less likely to do so in the presence of oxygen. So far as I am aware no fatality has yet been recorded under nitrous oxide and oxygen."

Having had one or two scares in my cases in 1903, in spite of my confidence in the method, I began to investigate what my colleague and teacher, Dr. T. S. Bennett, of New York, had to say upon the subject. Writing in the Reference Handbook of Medical Sciences in 1901, Dr. Bennett says the following:

"The writer has administered gas with air or with oxygen for operations lasting from a few minutes to more than two hours in several hundred cases, and while he has had no deaths, alarming states have several times appeared with such rapidity and so little warning that it seems probable that the general adoption of this form of anesthesia would lead to a mortality more nearly approximating, if not exceeding, that of chloroform and ether.

This opinion is strengthened by the occurrence of four such deaths in New York City which have come to the writer's notice within one year."

"It is therefore probable that nitrous oxide and oxygen is the safest general anesthetic if restricted to momentary administrations, as for dentistry. The prolonged administration of this anesthetic is quite a different matter, however, and at the present time there are not sufficient data of its use to warrant conclusions as to its safety."

Now here are two radically different opinions by two great authorities. Inasmuch as we are discussing its safety in surgery, and not in dentistry, we must give due weight to Dr. Bennett's words, especially as he is entirely a surgical and not a dental anesthetist.

A year ago last autumn I had a death under nitrous oxide and oxygen anesthesia. The patient was to be sure practically moribund, being pulseless and profoundly uraemic. Still the death did occur under gas and oxygen, and was

undoubtedly an anesthetic death, occurring during the first two or three minutes of the administration and before the operation was begun.

I have often noticed in my cases alarming asphyxial states, as spoken of by Bennett, develope with lightning-like rapidity, even when the patient was breathing large percentages of oxygen mixed with the nitrous oxide.

I regard gas and oxygen as a very valuable and in some cases almost ideal anesthetic. I think, however, it should always be administered by an expert anesthetist, and then with the utmost caution. Patients should invariably receive preliminary hypodermic medication with morphine, atropine, and in some cases scopolamine. The anesthetist must be allowed by the surgeon to have everything in his favor. The patient must be started not only on the operating table but actually in the position in which the operation is about to be performed. Plenty of time should be taken to induce anesthesia, and the surgeon must not make the incision until the patient is well under. The anesthetist must be allowed to freely observe the patient's face, and thus in head or goitre operations the view of the features must not be obstructed by covering the face-piece with towels, etc.

We are using at present at the hospital both a Hewitt and a Teter apparatus. These are both fitted with regulating stop cocks, which are supposed to provide for the admission to the mixing chamber of definite percentages of oxygen. I do not believe these devices are of any value, as the index for more or less oxygen is provided by the patient's color, and if sudden oxygenation is needed the regulating index must be entirely disregarded. I have not seen Dr. Gatch's apparatus, but should say from his description of it that it was eminently practical. Teter's apparatus, however, undoubtedly combines several features that are of great value.

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SYMPOSIUM ON ANESTHESIA.

ARTICLE VIII

NITROUS OXIDE-OXYGEN ANESTHESIA, WITH A DESCRIPTION OF A NEW APPARATUS.

By WALTER M. BOOTHBY, A.M., M.D.,
OF BOSTON.

READ JUNE 13, 1911.

I.—HISTORICAL REVIEW.

In December, 1844, or January, 1845, Dr. Horace Wells* of Hartford, Conn., on the invitation of Professor John Collins Warren addressed the latter's class in surgery on the subject of rendering the system insensible to pain during the performance of surgical operations; at the close of his remarks Wells invited the students to witness in the evening in a public hall, on Washington Street, an administration of nitrous oxide gas for this purpose to a boy whose tooth was to be extracted.

The exhibition took place, and although not entirely successful in abolishing pain, yet it deserves commemoration as being the first public attempt to demonstrate the fact that the body could safely be rendered insensible to pain for operative procedures. You will note that this trial preceded by nearly two years the public demonstration of the

* Wells, Horace: A History of the Discovery of the Application of Nitrous Oxide Gas, Ether, and other Vapors to Surgical operations. Hartford: J. Gaylord Wells, 1847. (The Boston Medical Library contains a copy which belonged to and was evidently annotated by W. J. Morton: 82. C. 87.)

anesthetic properties of ether by Morton* at the Massachusetts General Hospital on October 16, 1847.

I will delay you a moment with the very interesting account of the accidental discovery of the anesthetic properties of nitrous oxide: G. Q. Colton, a traveling entertainer of the type then common in New England, was giving an exhibition of the effect of "laughing gas" in the City of Hartford on December 10, 1844, of which he has given the following account:† "Among those who inhaled it was a young man by the name of Cooley, who, while under its influence, in jumping about, ran against some wooden benches or settees on the stage, bruising his legs badly. After taking his seat, he was astonished to find his legs bloody; and said he did not know that he had run against a bench, and felt no pain until after the effects of the gas had passed off. Dr. Wells, who sat next to him, noticed the circumstance; and, as the audience was retiring, asked me why a man could not have a tooth extracted without pain while under its influence. . . . The next morning—Dr. Riggs having been called in—I administered the gas to Dr. Wells and Dr. Riggs extracted a molar tooth for him. Dr. Wells on recovering, exclaimed: "It is the greatest discovery ever made! I didn't feel so much as the prick of a pin!"

In the next few days Wells used the gas successfully on ten or twelve cases; elated with his success he then went to Boston to demonstrate it with the result noted. On account of his practical failure in Boston his discovery attracted little attention. However the anesthetic property of nitrous oxide was recorded in literature in 1845 by the following statement of Ellsworth‡ in a comprehensive article "On the

* Warren, J. Mason: Inhalation of Ethereal Vapor for the Prevention of Pain in Surgical Operations. *Boston Medical and Surgical Journal*, 1846, XXXV. 375-379. Trustees Report Mass. Gen. Hosp, Boston, 1848.

† Colton, G. Q.: Anesthesia, Who Made and Developed this Great Discovery? Sherwood & Co., New York, 1886. (esp. pages 4 and 5.)

‡ Ellsworth: On the Modus Operandi of Medicine, page 396. *Boston Medical and Surgical Journal*, 1845, XXXII, 369-377; 389-400.

Modus Operandi of Medicine": "The nitrous oxide gas has been used in quite a number of cases by our own dentists, during the extraction of teeth, and has been found, by its excitement, perfectly to destroy pain. The patients appear very merry during the operation, and no unpleasant effects follow."

In spite of the attention attracted to it by the controversy* over the discovery of anesthesia which started after Morton's demonstration of ether no one seems to have attempted to confirm the value of nitrous oxide, and it passed into practical oblivion. For its revival we are indebted to our friend Colton, the traveling entertainer, from whose account I will again quote: "When in New Haven, in June, 1863, I was preparing to give an exhibition of the amusing effects of the gas, and invited a number of gentlemen to attend a private preliminary entertainment at which I gave a history of the discovery of anesthesia, and detailed the experiments of Wells; stating that I had never been able to induce a dentist to try the gas. Dr. J. H. Smith, a distinguished dentist of New Haven, who was present, said to me that he would be very glad to try the gas, provided I would administer it. I replied that I would be very glad to do it, as I wished to demonstrate what could be done with it. . . The first patient who came in was a wealthy old lady, for whom we extracted seven teeth with the gas. On recovering she was so pleased with the result, that she said I might mention her name to my audience. . . I did so. In three weeks and two days, we extracted a little over three thou-

* Ether Controversy: Report of Special Committee of House of Representatives, 1849, Thirtieth Congress—Report No. 114.

Jackson: First Practical Use of Ether in Surgical Operations—A Manuel of Etherization. Mansfield, Boston, 1861.

Sims: The Discovery of Anesthesia. Being a statement of the claims of Crawford W. Long to its first use in 1842. *Virginia Medical Monthly*, 1877, May.

Morton: Original depositions of sworn testimony deposited before Judge Putnam. Boston Medical Library, 1853.

sand teeth. This success induced me to abandon the 'Exhibition' business, and to establish an institution in New York called the Colton Dental Association. . . I have given it to over 150,000 patients without a single death. I have the autograph signatures, all numbered, of every patient in my office."*

From this beginning the use of nitrous oxide for the painless extraction of teeth spread rapidly in America, but very slowly in Europe until Colton demonstrated its value in Paris to Dr. Evans† in 1867; the latter, after a series of a thousand cases, visited England where he was invited to demonstrate its use at the Dental Hospital of London. Its use now spread with great rapidity, and naturally the best methods of administration were much discussed; in this connection I will quote to you the directions as given by Colton‡ to the editor of the *British Journal of Dental Science:* "Let a bag containing eight gallons of gas be used, and anesthesia is reached before the oxygen is absorbed and no ill effect attends the operation. The presence of a small amount of carbon dioxide in the bag, produced by breathing into it, produces no harm, *so long as there is an excess of oxygen.*"

The next year, 1868, Professor Andrews§ of the Chicago Medical School, appreciating the advantages of nitrous oxide as an anesthetic, proposed to substitute pure oxygen instead of air, thus rendering prolonged anesthesia possible. His article is very interesting as it consisted of an extended series of animal experiments which showed that nitrous oxide, to which one fifth part of oxygen had been added, could successfully maintain anesthesia for an extended time.

* Colton: ibid, pages 9 and 10.

† Evans, Parkinson, and others: Nitrous Oxide. *British Journal of Dental Science*, 1868, XI (see index).

‡ Colton: Experience in the Use of Nitrous Oxide. *British Journal of Dental Science*, 1868, XI, 253–257.

§ Andrews: Oxygen Mixture, A New Anesthetic Combination. *Chicago Medical Examiner*, 1868, IX, 656–661.

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However the paper attracted little attention and was apparently soon forgotten.

Not for ten years was another serious attempt made to use nitrous oxide for prolonged anesthesia; then in 1878 Professor Paul Bert* of Paris reasoned as follows: "The fact that nitrous oxide must be administered in a state of purity, signifies that in order to impregnate the organism with a sufficient quantity of the gas, its tension must be equal to the pressure of a single atmosphere . . . hence if we suppose the patient placed in an apparatus where the pressure can be raised to the equivalent of two atmospheres, the necessary tension will be secured by respiration of a gaseous mixture containing 50% nitrous oxide and 50% of air. . . Therefore as a consequence of the presence of a normal quantity of oxygen in the blood, all the natural conditions of respiration are maintained. . . Entering the cylindrical chamber, and increasing the atmospheric pressure by one fifth, I compel a dog to breath a mixture containing five sixths of nitrous oxide and one sixth of oxygen—a mixture in which it is evident that the tension of the gas is exactly equal to the tension of an atmosphere—which gives . . . a good anesthesia."

In 1881 Klikowitsch† demonstrated the value of nitrous oxide to relieve the pangs of labor; he combined nitrous oxide with oxygen in a large gasometer with the oxygen at about 20%, and administered it direct to the patient without increasing the pressure. Döderlein‡ in 1886, and

* Bert: Sur la possibilité d'obtenir à l'aide du protoxyde d'azote, une insensibilité de longue durée et sur l'innocuité de cet anesthésie. Comtes rendus, 1878, Nov. 11, p. 728.

Gazette medicale de Paris, 1878, 108, 498, 579.

Lutand: L'anesthésie par le protoxyde d'ozote sous tension. Méthode de M. Paul Bert. *Gaz. hebdom*: 1879, XXVI. 14, 213–214; 39, 613–614.

† Klikowitsch: Ueber das Stickstoffoxydul als Anaestheticum bei Geburten. Arch. f. Gynakologie, 1881, XVIII. 81–108.

‡ Döderlein: Ueber Stickoxydul-Sauerstoffanäesthesie. Arch. f. Gynakologie, 1886, XXVII. 85–101.

Hillisher* in 1887 continued the work and confirmed the results of Klikowitsch. At the same time Hewitt† was studying nitrous oxide anesthesia in England, and in 1886 published a study of the question of rebreathing so strongly advocated by many users especially Coleman‡ (1868); his conclusion was that it was permissible only to a slight extent. Somewhat later, becoming acquainted with the work of Hillisher, Hewitt studied the "Effects produced in the Human Subject by the Administration of Definite Mixtures of Nitrous Oxide and Air, and of Nitrous Oxide and Oxygen"; this he published in 1889, followed in 1892 by a more extended account, with the description of a very satisfactory apparatus.

From this time on nitrous oxide and oxygen have been used in some clinics more or less frequently for general surgical work. Wherever such use has been at all extended, the most favorable reports of the great value of this form of anesthesia have been made. Its use seems to depend on the availability of an anesthetist willing to master the mechanical difficulties of producing a smooth surgical narcosis by means of any of the existing apparatuses. Gatch§ of Johns Hopkins has so far obtained the best results largely because he understands the physiology of breathing as demonstrated by Haldane and his co-workers in England and also appreciates the importance of carbon dioxide in its relation to shock as shown by Henderson of Yale.

* Hillisher: Ueber die Verwendung der Stickstoffoxydulsauerstoff gemenges zu Narkosen. Deut. Monatschrift f. Zahnheilkunde, 1887 (Sept.); Centralblatt f. Chir., (1888, XV. 115).

† Hewitt: An Inquiry into the Several Methods of Administering Nitrous Oxide. *British Dental Assoc. Jour.*, 1886, VII. 86-90; 152-160; 208-215.

Hewitt: On the Anæsthetic Effect of Nitrous Oxide with Oxygen at Ordinary Atmospheric Pressures. Transactions Odontological Soc., 1892, XXIV. 8, 194-244.

‡ Coleman: Re-inhalation of Nitrous Oxide. *British Med. Jour.*, 1868, I. 410; II. 114-115. St. Bartholomew's Hosp. Reports, 1869, V. 153-164.

§ Gatch: Nitrous Oxide Anæsthesia by the Method of Rebreathing. *Jour. Am. Med. Ass.*, 1910, LIV. 775-780.

Likewise Miss Hodgins,* the anesthetist to the Lakeside Hospital, Cleveland, under the direction of Crile, has been successful. Her success I attribute in a large part to the use of gases under moderate tension, and at room temperature; this is due to the fact that the gases are manufactured in the cellar of the Hospital and piped to the operating room under moderate pressure (about 30 lbs.).

II.—SUMMARY OF THE PHYSIOLOGICAL AND CLINICAL FACTS TO BE CONSIDERED IN MODERN SURGICAL ANESTHESIA.

To-day, nitrous oxide-oxygen anesthesia is generally credited as being the method of choice provided it be found practicable from a mechanical standpoint by those who have made a study of the subject. The physiological and clinical facts forming the basis of this opinion may be summarized under the following seven heads:

(1) Safety of the patient.
(2) Complete surgical relaxation.
(3) Agreeableness and rapidity of induction.
(4) Agreeableness and rapidity of recovery.
(5) Tendency to the avoidance of shock.
(6) Tendency to the avoidance of post-operative distension or ileus.
(7) Absence of post-operative complications.

(1) Statistical studies lead us to the conclusion that nitrous oxide is the safest of anesthetics and is recommended even where ether cannot be used. Ether is the next in safety.

(2) Complete surgical relaxation can usually be obtained by gas and oxygen alone; however, it can always be obtained by the addition of very minute quantities of ether vapor; the quantity needed is so small that it has hardly an appreciable after-effect; that is, it does not produce post-operative nausea.

* Hodgins: Personal communication to Dr. F. J. Cotton, Boston.

(3) The agreeableness and rapidity of the induction of nitrous oxide anesthesia are too well known to need more than mention.

(4) The rapidity as well as the minimal discomfort of recovery are also known to you all, at least by heresay, to be convinced of the great difference from that of the recovery from an ether anesthesia; all that one needs to do is to talk with some patient who has experienced both methods. In this connection Dr. Cotton, who instigated my work on this subject, informs me that in Cleveland, according to Dr. Crile,* the patients are actually beginning to demand its use.

(5) The question of shock is of such importance that I wish time allowed me to do more than mention the points to be considered: As you know, the most generally accepted hypothesis of shock is that of exhaustion of the vasomotor centre; perhaps you also know that this hypothesis has been shown experimentally to be untenable because the clinical picture of shock and the characteristic fall of blood pressure may be present while the vasomotor centre still gives a normal reflex response to the stimulation of afferent nerves. Moreover, it has been shown that the peripheral vessels are not dilated in shock, as they would be were the vasomotor centre exhausted; on the contrary they are constricted.

Coincidentally Haldane and his colleagues in England have shown the importance of the carbon dioxide content of the blood in regard to the regulation of respiration. Very recently Henderson, of Yale, has shown that the CO_2 content of the blood is also, probably, an important element in the regulation of blood pressure, perhaps by its influence on the tonicity of the veins; that is, as Henderson maintains, a decrease of the CO_2 content of the blood by excessive respiration causes the veins to dilate and the production of the picture of shock. The point here to be made is that ether as it is usually given does cause excessive respiration, and excessive respiration causes a reduction of the CO_2 content

* Personal communication to Dr. Cotton.

of the blood. One advantage of nitrous oxide anesthesia is that the respiration is less disturbed; however, if deep and frequent respirations are caused by the peripheral irritation of the operative procedures their tendency to produce an injurious reduction of the CO_2 content can be offset by means of a slight but constant rebreathing.

The much decreased tendency to the production of shock by an operation under nitrous oxide-oxygen anesthesia can easily be demonstrated to any one who will carefully watch a few cases. I here wish to emphasize the point that I do not imply that a skilfully conducted ether anesthesia produces a picture of profound shock; what I do mean is that the picture of ether recovery as represented by an increase in the pulse rate, wet clammy skin, marked pallor with most distressing nausea and vomiting lasting twenty-four to forty-eight hours, producing an almost absolute loss of morale, are all reduced and ameliorated when nitrous oxide-oxygen has been the anesthetic.

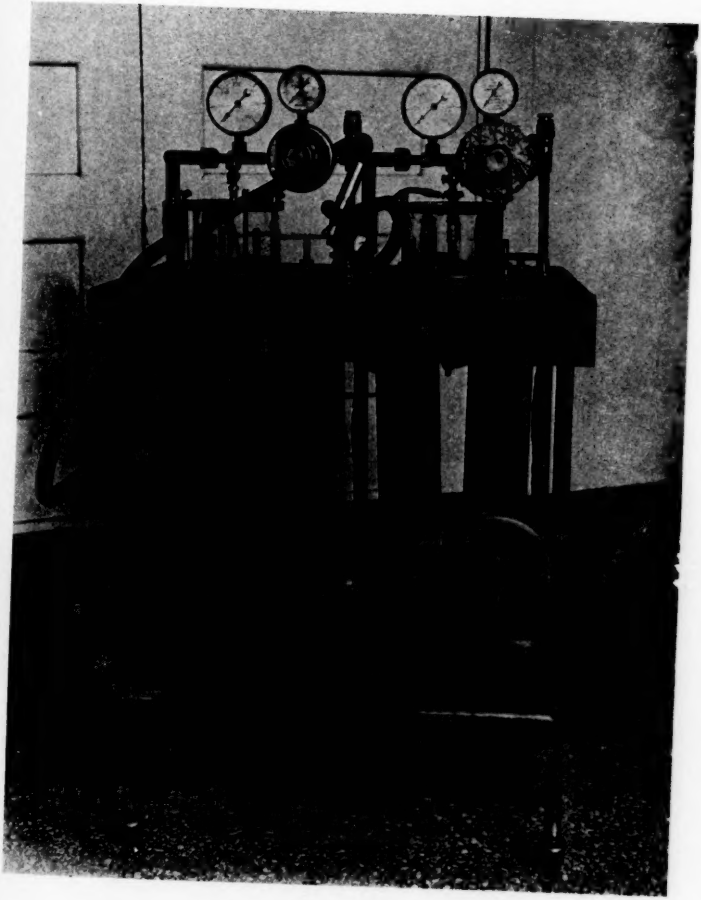
(6) Henderson has also shown that peristalsis is decreased by lowering the CO_2 content of the blood; if this observation is confirmed it would explain the clinical fact that after a nitrous oxide-oxygen anesthesia the patient suffers less from gas-pains, distension, and abdominal distress than after the ordinary administration of ether.

(7) Post-operative complications, so far as we can judge, will be greatly reduced. This will be especially true of those of the lung on account of the practicability of efficiently warming the gases.*

III.—DESCRIPTION OF A NEW APPARATUS WHICH ELIMINATES THE MECHANICAL AND OPERATIVE DIFFICULTIES OF ADMINISTRATION.

One of the chief reasons why nitrous oxide-oxygen anes-

* References to the literature on which the above is based can be found in the recent articles of Henderson in the *American Journal of Physiology.*



thesia has not in the past been more generally used has been the mechanical difficulties attending the same. For when I took up this subject last year, I could find no apparatus described in literature or on the market (aside from the manufacture of the gases on the premises) which would meet the fundamental requirement: a smooth even flow of the two gases. That this even flow is essential even a brief consideration of the matter renders evident, for only by such an even flow can one obtain a uniform mixture to administer to the patient. Further, the flow of the respective gases must be regulated easily in order to bring the mixture to the desired proportions of nitrous oxide and oxygen. Likewise it is essential that adequate provision be made for the addition of ether vapor to be able in all cases to obtain complete relaxation. A less vital though still important point is warming the gases.

My interest in taking up this matter practically as a surgical method was first focused by the enthusiam of Dr. F. J. Cotton on his return from an observation (of some days duration) of the surprising results daily shown in Crile's clinic in Cleveland. Accordingly, at the suggestion of Dr. Cotton and with his help and that of Dr. Albert Ehrenfried, I designed and had built an apparatus which meets all these conditions. The important points I will now go over in more detail.

In the first place both gases are taken from high pressure steel cylinders, which in the case of nitrous oxide have a pressure of about 700 pounds to the square inch, and in the case of oxygen the pressure is sometimes as high as 2,000 pounds to the square inch; truly excessive pressures from which to attempt to give a smooth flow of gases to a human being and expect an even surgical anesthesia. To obviate this difficulty it is necessary to reduce this pressure automatically by means of a reducing valve to a working pressure of about twenty pounds to the square inch; a pressure

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from which the volume of each gas desired can be easily regulated by means of a hand valve.

In the second place the use of reducing valves introduces a mechanical complication which has to be overcome; namely the formation of a connection between the tanks and the apparatus without leakage; also provision must be made for the renewal of tanks without interrupting the use of the apparatus. This is obtained by a specially designed casting consisting of four yokes arranged in pairs so that two nitrous oxide and two oxygen tanks can be clamped on together with appropriate cutout valves and drill holes leading to the reducing valves. To insure the nitrous oxide reducing valve from freezing it is surrounded by an electric heating coil.

Thirdly, it is essential to be able to judge of the amount of each gas that is being used, for otherwise it is impossible to even approximate a correct mixture. This knowledge is obtained by passing the gases through water into a mixing chamber; the rate of the bubbling of the gas through the water gives a very accurate index of the percentages of the two gases as administered to the patient. In addition, this procedure adds a desirable degree of moisture to the gases.

The fourth important point is the arrangement for the addition of the desired amount of ether vapor that is necessary in some cases to obtain complete relaxation. As the amount varies within wide limits there is provided an ether chamber of large evaporating area and a three way exit valve. This valve is so arranged that when closed no ether is added to the gas mixture going to the patient. By opening it gradually, an increasing amount of the nitrous oxide-oxygen mixture can be made to pass over the surface of the ether; should occasion ever arise for an extremely strong ether percentage the valve when pushed still further causes the gases to bubble through the ether. This valve is placed very handily beside the oxygen valve, as these two are the only ones that need attention during the progress of an anesthesia.

The fifth consideration is that of efficiently warming the anesthetic mixture just before inhalation. This is conveniently obtained by passing the mixture through an electric heater which delivers the gases at a temperature about halfway between the room temperature and the body temperature.

Lastly, the question of rebreathing is taken into account by arranging the inspiratory or inlet valve on the mask so that its action can be partly or entirely eliminated, while at the same time the expiratory or outlet valve is arranged with a spring which, by an increase in tension, partially or completely closes the valve. Thus we may control rebreathing to any proportion of the respiratory volume. By this means each expiration may be partly rebreathed; the amount of the gases that escape through the expiratory valve are replaced by a corresponding amount of fresh gases; by this partial method of rebreathing a smoother CO_2 regulation is maintained than when the intermittent complete rebreathing method is used. This point is very important in the prevention of CO_2 loss from an excessive respiration due to peripheral irritation as pointed out in the previous section. It is also of direct benefit in the production of a relaxed anesthesia under nitrous oxide, because a large amount of this gas as well as oxygen must be absorbed for this purpose. As Haldane and others have pointed out, a slight increase in the CO_2 percentage of the inspired air doubles the volume of the air or mixture inhaled by stimulation of the respiratory center, consequently this increased depth and rate of respiration renders more nitrous oxide and oxygen available to the blood. (This must not be confused with an increased depth and rate of respiration which is a reflex result of peripheral irritation and which causes an undesirable reduction of the CO_2 content, unless provision is made to offset the same by rebreathing.)

In addition the apparatus is equipped with an electric motor and air pump for use when air is desired as a carrier for

ether in such cases as are found unsuitable for gas-oxygen anesthesia. Whether or not, for intratracheal work, the the gas-oxygen ether or the air ether mixture will be found most suitable is yet to be determined. For the use of the intratracheal method there is a separate connection from that to the mask, in the circuit of which is placed a mercury safety-valve to prevent the danger of undue intrathoracic pressure; a danger which without question must be efficiently overcome.

At first glance this apparatus may look complicated and difficult to use, and therefore like its predecessors appear impracticable. Such is, however, not the case, as these seeming complications remove from the anesthetiser all operative difficulties; in fact they need no attention, thus leaving him free to give his whole thought to the patient. After becoming acquainted with the nitrous oxide-oxygen ratio of the patient the hand valves need not be touched for intervals of 5 or, sometimes, 10 minutes. The details of operative technic I will reserve for a future paper.

IV.—CONCLUSIONS.

(1) The first attempt to give a public demonstration that the body could be safely rendered insensible to pain for surgical procedures was made in Boston by Dr. Horace Wells, a dentist of Hartford, Conn., with nitrous oxide in December, 1844, or January, 1845, nearly two years before the demonstration of ether by Morton at the Massachusetts General Hospital, on October 16, 1846.

(2) Nitrous oxide-oxygen anesthesia, with the addition of such minimal quantities of ether vapor as may be needed, is the anesthetic of choice.

(3) The general use of this anesthetic is dependent on the perfection of an apparatus which overcomes the mechanical and operative difficulties attending its administration.

(4) Such an apparatus is described.

SYMPOSIUM ON ANESTHESIA.

ARTICLE IX.

NITROUS OXIDE AND OXYGEN ANESTHESIA.

By A. H. MANDELL, M.D.
OF NEW BEDFORD.

READ JUNE 13, 1911.

It is not my purpose in this paper to enter into the history of nitrous oxide oxygen anesthesia, or to dilate on the work of others; but to present my reasons for using it, and give, briefly, a description of the apparatus used, and portions of my experience.

Let it be understood now that the terms nitrous oxide and gas-oxygen mean nitrous oxide and oxygen in every instance.

After hearing the report of the Anesthesia Commission of the A. M. A. at St. Louis last year the writer went to Cleveland to observe Crile's work with nitrous oxide, and a little later to the Johns Hopkins Hospital for further investigation. What he learned at these clinics convinced him that nitrous oxide was superior to ether as an anesthetic for general use.

If you will grant that ether by the drop-method is the only rival of gas-oxygen, the following are some of the reasons for claiming superiority of the latter.

Crile's experiments have proven that ether produces a demonstrable lesion of the central nerve cells. Gas-oxygen has almost no appreciable effect.

Hamburg and Ewing have shown that ether reduces the color index of the blood quite constantly, continuing until

the sixth day, while with gas-oxygen the color index drops slightly when the mask is removed and continues for about two hours when it usually ceases.

There is a constant lowering of blood pressure under ether, while there is almost none under gas-oxygen.

There is no irritation of the mucus membranes of the larynx or bronchi with gas-oxygen, while the effect of ether on these membranes is distinctly contrary.

Patients taking gas-oxygen usually recover consciousness in from one to three minutes with little or no nausea, and their ultimate recovery is less stormy.

With gas-oxygen there is no deleterious effect on the liver or kidneys after long administration, as is frequently the case with ether.

In my experience, those who have taken both anesthetics very much prefer gas-oxygen.

In competent hands it is the safer anesthetic.

Dr. Frederick Hewitt of London, an authority on anesthetics, writes:—"There is no form of anæsthesia at present known which is so devoid of danger as that which results from nitrous oxide when administered with a sufficient percentage of oxygen to prevent all asphyxial complications."

The Anesthesia Commission of the A. M. A. reported last year at St. Louis in regard to gas-oxygen, "In the hands of highly skilled anesthetists the method is the best yet devised."

The writer has used gas-oxygen since last October in nearly 200 cases.

The apparatus employed is the Davis.

EXHIBIT.

This apparatus seemed less complicated than the Teter and was preferred to the Gatch, only because the Davis has two bags, one for gas and one for oxygen, enabling the operator to stop gas instantly and give pure oxygen.

The cases in which I have used gas-oxygen include nearly the whole range of surgery, except nose and throat and brain work.

In many instances it has been necessary to give a drachm or two of ether with the gas (as the Davis apparatus permits) to produce the required amount of muscular relaxation. The patient is not cognizant of the addition, and this small amount of ether does not cause nausea or vomitting.

All patients are given $\frac{1}{6}$ gr. morphine sulphate and $\frac{1}{150}$ gr. scopolomine two hours before operation.

The proportions of nitrous oxide and oxygen vary in every case. Give enough oxygen to keep the patient's color good is the only rule.

In the total number of cases there were only two in which gas-oxygen could not be taken because of persistent cyanosis, and ether was substituted. In neither of these two instances was ether borne well.

The longest case, a tedious breast operation, lasted $3\frac{1}{2}$ hours, and when the patient regained consciousness she said she felt as though she had awakened from sleep.

Nausea and vomiting, when present after gas-oxygen, seem to depend on the operation, and not on the anesthetic. Curettage nearly always causes vomiting, and after gall bladder operations nearly 50 percent are nauseated.

Our experience has demonstrated that the surgeon can, by delicacy in manipulation, materially diminish the number of cases in which it is necessary to add ether to the nitrous oxide. A sharp pull with a retractor will cause the abdominal muscles to contract when steady pressure will not disturb.

The abdominal viscera must be handled gently if one is to obtain a quiet field for operation and avoid the use of ether. This will be readily understood when we remember that gas-oxygen produces a light anesthesia.

Nitrous oxide is much more difficult to administer than is ether.

It is no easier, possibly not so easy, for the surgeon to operate under nitrous oxide as it is to operate under ether, but to the patient the advantages of nitrous oxide over ether are distinct, and may be clearly evidenced by the following facts:—

(1) That he loses consciousness in three or four breaths.
(2) That he has no strangling or choking.
(3) That he recovers consciousness immediately when the anesthetic is stopped.
(4) That he has little, usually no, nausea or vomiting.
(5) That he can take liquids much sooner after operation.
(6) That his ultimate recovery is shortened.

SYMPOSIUM ON ANESTHESIA.

ARTICLE X.

INTRATRACHEAL INSUFFLATION AS AN ANESTHETIC METHOD.

By WILLIAM C. QUINBY, M.D.
OF BOSTON.

READ JUNE 13, 1911.

Among the various ways of administering anesthetics the intratracheal method has recently become one of importance. Therefore a brief description of the method, and what is accomplished by it, will be of interest at this meeting.

The essential features in which this method differs from the ordinary anesthesia with a cone are that by means of a tube passed into the trachea, and fitting it loosely, ether-laden air is delivered at or just above the bifurcation of the trachea, in a nearly constant stream from a bellows or other source.

When this is done there follows a very important phenomenon. The to and fro respiratory movements become markedly fewer in number, until with a sufficiently large volume of air sent through the tube, respiratory movement ceases entirely. The heart beat and blood pressure, however, continue in a perfectly normal manner, practically unaltered. In other words, a condition has been produced in which the usual to and fro respiratory movements are no longer necessary for maintainance of life, and in which the surgical entrance to either one or both sides of the thorax is possible, without the occurrence of any ill effects from the ensuing pneumothorax.

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This is the method of artificial ventilation of the body which Meltzer, its discoverer, has named intratracheal insufflation. It will be of value to examine for a moment the physiological basis of the method.

During normal life the respiratory exchange is maintained by an inspiratory current carrying in oxygen, alternating with an expiratory current carrying out carbon dioxide. This type of breathing is termed by the physiologists *external respiration* to distinguish it from the exchange of gases which takes place between the carbon dioxide of the tissues and the oxygen of the blood flowing continuously through a capillary, which is called *internal respiration.* Through the work of Meltzer we are in possession of a new physiological fact, *i. e.* that for the normal type of external respiration, a type similar to internal respiration may be substituted by the continuous passage of air over the alveoli; and during which life is as safe in all particulars as under the usual to and fro type.

Working with Dr. G. W. Morse in the Surgical Research Laboratory of the Harvard Medical School last summer, I made various physiological observations, using this method of anesthesia on dogs, the results of which were detailed in "Surgery Gynecology and Obstetrics" of Nov. 1910. It was shown that the rate and regularity of the heart beat were unaltered. The peripheral and pulmonary blood pressures were measured and found to be also uninfluenced. Inhibition of the heart due to vagus reflexes set up by the passage of the tube was either not seen, or could be quickly controlled by the use of atropin. No injury resulted to the larynx or trachea from the passage of the tube, and there was no pneumonia following its use. Histological examination of the lungs showed them to be normal, and free from capillary hemorrhage or emphysema.

Besides these findings, two facts early demonstrated by Meltzer complete the necessary evidence as to the safety of

the method. The first is that when all the voluntary muscles of an animal are paralyzed by a dose of curare, insufflation by the tracheal tube will preserve life in a normal manner, till at the end of several hours the curare has been excreted. The other is, that even with the animal's mouth filled with vomitus, the return current of air coming out around the tube is able to prevent its entrance below the larynx.

Such facts as these make it evident that the method of intratracheal insufflation rests on a safe physiological basis.

The next question to be decided is that of the safety and practicability of the method when applied to man. Within the past year Dr. Elsberg of New York has reported the successful administration of ether by this method in something over 100 cases. In Boston, Drs. Ehrenfried and Boothby, as well as myself have used it. I suppose upwards of 150 cases in all have been subjected to anesthesia by the intratracheal route, and we have yet to hear of any untoward result. There has been no pneumonia caused by it.

The first and most important advantage of this intratracheal insufflation is found in its use during surgical operations which open the chest cavity: for in contradistinction to all other methods yet used, this is the only one in which proper oxygenation of the blood is performed independently of any respiratory effort on the part of the patient. With it the usual type of external respiration is converted into the internal type. In all the positive and negative pressure contrivances, expiration takes place at a disadvantage, the barometric pressure which exists on the surface of the exposed lung being less than that within the air spaces. This throws an increased amount of work on the right heart which must compensate. Besides this, there is a tendency for carbon dioxide to remain too long in the alveolar spaces to the exclusion of the better oxygenated air. In the course

of a long operation this has a definite poisoning effect on the heart wall. Both these undesirable effects are avoided by the insufflation method.

Furthermore, the much decreased number of respiratory movements makes operations within the depths of the thorax much easier than they would be otherwise. Another advantage is that the gullet is left free for instrumentation. This important point was illustrated very well in a case which I etherized by the intratracheal method for Dr. Scudder. The patient suffered from a rupture of the diaphragm; the left thorax containing the whole stomach, a piece of the transverse colon, and a portion of the great omentum. During the operation it became necessary to empty the stomach of a quantity of fluid which it was found to contain, so that it might be made small enough to pass back through the rent in the diaphragm. Without inconveniencing the smooth course of the anesthesia in the least, a stomach tube was passed. The patient made a perfect convalesence.

Still another advantage of the intratracheal method is the very complete æration produced. Patients recover from the anesthetic very quickly as a consequence of this.

Then, too, though it does not apply to anesthesia as such, I cannot forbear to mention the great importance of the insufflation method, using plain air or oxygen, for cases of cessation of respiration from any cause. The lives of patients suffering from brain abscess and morphia poisoning have already been saved by it. It is the best method known which will "breathe for the patient." Dr. Meltzer has had even this experience. He has given a dog four times the lethal dose of strychnia, then prevented the convulsions by the injection of curare, and by means of the intratracheal insufflation of air, kept up a good ventilation till both the strychnia and the curare were excreted and the animal recovered.

In regard to the clinical application of anesthesia by this

method, there is little to add to the directions given in my previous paper of last November. The air should be warmed and moistened before it reaches the bifurcation of the trachea. It must also contain ether in varying amounts. Any apparatus which will fulfil these requisites and is simple, is quite adequate. The apparatus which I have had made for me by Codman & Shurtleff has thus far been entirely satisfactory.

The tracheal tubes are of soft rubber, with perfectly smooth surface free from seams, about 15 inches long, with finished ends like stomach tubes. Their wall is of a uniform thickness of 1mm. and for convenience in judging the length of tube which one is using, they should be marked in centimeters from 15 to 30.

Before introduction, the size of the trachea above the sternal notch should be estimated and a tube chosen which is about half this size, or a little larger. After a hypodermic of morphin and atropin, the patient should be etherized in the usual way. Then under full surgical anesthesia, with the patient in the Rose position, the mouth gag is inserted and the sterile tracheal tube introduced, either by a laryngeal forceps, the epiglottis being held forward by the operator's left forefinger, or by the use of a Killian speculum. Previous painting of the pharynx and larynx with cocain I have found to be unnecessary. After passing the glottis, the tube is pushed forward till a resistance is felt, or until 26cm. have been introduced (in male adults) as measured against the upper incisors. It should then be withdrawn about 1cm. so as to lie just above the bifurcation, and then connected with the rest of the apparatus. When it is correctly placed air can be heard to enter both lungs on auscultation.

It is my hope that many surgeons will become familiar with the principles underlying this method, as it is one of undoubted value, and promises to be of great aid in the future of thoracic surgery.

SYMPOSIUM ON ANESTHESIA.

ARTICLE XI.

AN APPARATUS FOR THE ADMINISTRATION OF ETHER BY THE INTRATHORACIC INSUFFLATION.

By ALBERT EHRENFRIED, M.D.
OF BOSTON.

READ JUNE 13, 1911.

The apparatus which I have here is a simple apparatus of my own devising, which I have been using now for several months for the administration of ether by the intrathoracic insufflation method. I described it and cited some cases in the *Boston Medical and Surgical Journal* of April 13th. It is an outgrowth of an experience with the method in animal experimentation, extending over several months.

Intratracheal insufflation anesthesia was originated by Meltzer and Auer; it was first adapted to practical surgery by Elsberg, also of New York, who within a few days has reported a series of 100 cases. I believe that I am the first to employ the method successfully in human surgery outside of New York, and I have used it now on ten cases.

The apparatus which Elsberg has elaborated is probably the best for the purpose, but it has many drawbacks. It is heavy, cumbersome and expensive, and depends upon an electric current for its motive force, and needs an expert to run it. My apparatus, on the contrary, is light, simple and of low cost, yet it is entirely adequate and can be used anywhere that ether can be given. In two of my cases the apparatus was run successfully by men who had never seen it before. In addition, it has some minor points of advantage over the

Elsberg machine,—it can, on occasion, deliver a highly concentrated ether vapor (Elsberg has mentioned a case or cases which he could not anesthetize with his apparatus); it is provided with a device to prevent the possibility of any droplets of ether vapor which have condensed in passage through the tube being carried into the lungs; it is provided with a safety valve by which the intrathoracic pressure can be controlled and which acts as a positive safeguard against any injurious effects of temporary or prolonged over-pressure on the lung tissue.

This appliance I consider of essential importance, particularly for men who are using the apparatus without much previous experience. Patients going under may at times exhibit spasm of the glottis, and the same phenomenon is apt to appear when fresh ether vapor is thrown into the circuit. At such times the safety-valve blows off, and releases what might become a dangerous intrathoracic pressure.

I use an ordinary French lisle catheter, size 20 to 24, for the intratracheal tube. At first I tried introducing it upon my finger, as one would a stomach tube, the patient being anesthetized, but I gave this up as unsatisfactory. Next, I followed the suggestion of Elsberg and made use of an electrically illuminated Chevalier Jackson laryngoscope. This worked well when it worked. It had, however, the inconvenience of all electrically lighted specula, and it possessed the great disadvantage of making it necessary to change the patient on the table to and from the Rose position. In other words, the patient could not be scrubbed up and the operative field prepared until the tube was in place. Finally, following a suggestion of Dr. F. J. Cotton, I evolved an introducer. This, as you see, is a simple clamp, built with a curve like that of the O'Dwyer intuber, and made to grasp the tube near its extremity.

This little instrument has given me great satisfaction. With it the tube can ordinarily be introduced within a few

seconds, at any time during the operation, without changing the patient on the table.

This method of anesthesia has had its tryout in Boston, and has demonstrated its value. My series is not, of course, so large as that of Elsberg, but it has been ample to show the class of cases to which the method can be applied with advantage. It includes amputation of the tongue for cancer, dissections of the neck, dissection of the tonsil, operations about the larynx, as well as intrathoracic cases. The operator is at first somewhat disconcerted by the appearance of the patient, who lies upon the table motionless, with suspended respiration, and without making a sound, but this is overcome by the comfort of knowing that the patient is bound to get sufficient air under all circumstances, that no blood or mucus can be aspirated into the lungs, that a minimum of ether is being used, and that there will be little or no after-sickness. Added to this is the great convenience, in operations about the face, neck and thorax, of not being hampered by the close presence and constant ministrations of the man with the ether-cone.

DISCUSSION OF THE PRECEDING SIX PAPERS.

Dr. G. P. Twitchell, Greenfield: The various forms of apparatus which have been shown us to-day, for the induction of gas and oxygen anesthesia, are very interesting and instructive. But to the general practitioner, particularly to one practising in the country, they hardly appeal, on account of the expense and difficulty in transferring the apparatus from place to place, which tends to prohibit the general adoption of this method. The anesthetic which is the safest and which can be administered in the simplest form, it seems to me, is the most desirable, and ether, administered by the open drop method, in my estimation, fulfils this requirement.

For the last two years I have used it in this way, almost exclusively, with very satisfactory results. This method has

practically eliminated the preliminary feeling of choking and strangulation, the stage of excitement, and nausea and vomiting. Cyanosis is a symptom which has not occurred in any of the cases where I have used the open drop method and the use of the mouth gag and tongue forceps has not been called for.

In order to get the best results in the administration of ether, there are many elements which are of vital importance. It is necessary to secure the confidence of your patient and to eliminate, as far as possible, fear and anxiety. The room in which the anesthetic is administered should be kept absolutely quiet. Conversation around the table, by nurses or attendants, is distracting to the patient and interferes, in my experience, with the successful use of the anesthetic. The anesthetizer can with advantage keep up a conversation with his patient, tending to reassure him that everything is going right. In order to aid in bringing about this tranquillity, rather than for any other effect which it produces, I have been in the habit of giving a hypodermic of morphia and atropia half an hour before I begin the anesthesia, and, with me, it has proved very satisfactory and has materially decreased the amount of ether required.

In the cases in the last two years in which I have administered ether by the open drop method, following a hypodermic of morphia and atropia, the average amount required to produce anesthesia has been from 2 to 2½ ounces and under 5 ounces per hour has been needed to maintain the anesthesia during the operation. This small amount, I feel, materially lessens the shock to the patient in comparison with the large amount which I formerly found it necessary to use with the cone method. The continuous dropping is a very important feature, not putting the ether on in small amounts, every little while, but dropping continuously, simply varying the rapidity with which the ether is dropped. The less profound the anesthesia, the better for the patient, and here it is important, to bring about this result, that there should be team work between the operator and the anesthetizer. After the patient is under the anesthetic, if you know your operator and are familiar with his work and can, without neglecting your patient, keep track of the structures which he is handling, so that, when there is no special sensitiveness about the region on which he is working, you can cut down

on your anesthesia and increase it when he is about to approach, in his manipulations, a more sensitive part, you can get better results.

I have had patients, after abdominal operations, come out of the anesthesia enough during the dressing so that they were able to help in changing their position on the table, and by the time they were in bed were practically out of the ether. Another thing that possibly some of us neglect, is to be absolutely sure of the freshness of the ether that we are using. A can of ether which has been opened and exposed to the air is not fit to be used as a general anesthetic.

DR. F. J. COTTON, Boston: I have been very much interested in this anesthesia work. In the first place, we all realize that ether is not as satisfactory as a routine anesthetic as we were brought up to think; there are certainly some disadvantages, and I have seen some lung complications that were due to ether; I believe they would not have happened if we had not etherized. Last fall I became very much interested in nitrous oxide and I went to Cleveland to see the work of Dr. Crile and Miss Hodgins. I think she is the most expert anesthetizer we have; she has had a good laboratory training and has worked out wonderful results. I saw a great deal of routine nitrous oxide administration—I think Crile has had 2,000 cases without any untoward results that were due to the anesthetic.

In the first place they had there eliminated the particular objection, and that is the straining respiration; they have found that straining respiration can be controlled if you regard it as a reflex of asphyxia, or of too much gas, which comes to the same thing. If you put in ether at proper intervals you can avoid the straining so that abdominal work can be done as well as other work. But in working out there they are aided by the low tension of the gas; it comes up from the cellar under twenty-five pounds pressure, while here we have a very high initial tension.

I got Dr. Boothby interested in this work and he has incorporated this low tension scheme and many others in the apparatus shown.

I believe this gas-oxygen is the coming anesthesia. I have not used any other lately except in cases where we could not get the patients to the instrument, which is not transportable, and this is a disadvantage.

I think shock is very much minimized. Personally, it strikes me that my patients come out very well; they have a warm, pink, dry skin, and do not show the shock in severe cases. I have used this gas for two months, in severe belly cases and many others, without their showing any results of the operation at all. The vomiting and headache are comparatively mild and comparatively infrequent.

About morphine—I question whether the routine giving of morphine and atropine does not interfere with the work.

Of course, this method must be in the hands of experts; this is a complicated machine and it is not a thing which is going to be popular. In the other hospitals in Cleveland the doctors are protesting about its adoption being forced, because the patients are insisting that they have got to be anesthetized in this way.

About the intratracheal method—it seems to me that is our way out in a great deal of surgery of the mouth and throat. I have used it in the case of cancer of the tongue, and I am inclined to think that the point of the return current of air is going, in most cases, to entirely eliminate the pneumonia; the patients do not get a chance to swallow anything in the trachea. There is one point about it I will mention, and that is, it is disconcerting to the operator to have a patient who is not breathing. It seems to me that the intratracheal method and nitrous oxide method are both very interesting and extremely important.

Dr. F. L. Richardson, Boston: This is an apparatus which I have used for intratracheal work, and it is essentially like the apparatus which you have seen before. It consists, as Dr. Ehrenfried's and Dr. Boothby's machines do, of a Wolff bottle which contains ether. I have also added a filter, because I take my air from near the floor and I was afraid it might contain dust. The air goes from the filter into this tube, and may either go directly to the heater coil or, by regulating the stop-cocks, any part of it may be passed over the ether in the Wolff bottle. The heater coil I consider a rather important addition, as it has been found that warmed ether vapor is much less irritating than cold vapor. Both the Wolff bottle and the heater coil are immersed in a dish of warm water, which may be kept warm by an electric lamp partly immersed in the water. From

the heater coil the vapor goes to a manometer and blow-off, which I do not consider absolutely necessary, but they are a comfort to the man who uses the apparatus. The method of using this apparatus is essentially the same as that already described.

Dr. F. B. Lund, Boston: I wish to say that Dr. Richardson used this apparatus in a case for me and the patient did not breathe well, but he is in splendid condition, and we did not have any shock. Dr. Cotton says that the use of nitrous oxide eliminates shock, but we did not have any shock, and I do not think it is necessary to use nitrous oxide in order to eliminate shock.

Dr. J. B. Blake, Boston: I think the advances which these various forms of apparatus suggest are very desirable, a sort of work which ought to be encouraged in a center as large as Boston and in the country which is the birth-place of anesthesia; the men who put their time and their money into this work ought not to be forgotten.

Such work, however, except in the hands of experts is dangerous, and a great deal of the surgery must be done with the average non-expert anesthetizer; he must give it in the same way we have been in the habit of having it given, in a simple cone, in something like the original ether sponge. It seems to me that the men who give ether do not realize the dignity of the position of anesthetizer. The anesthetizer is a most important person, he is sometimes more important than the first assistant, at times more important than the operator himself, and yet you will often see the etherizer looking around the room and not paying attention to the patient. All that he knows and all that he ever will know is worthy of being bestowed upon the patient who is under his care, and the medical student should be taught to give ether carefully; his responsibility is great and he must master his simple apparatus before he can go on to the more complicated ones.

It will be a great many years before any combination or sequence may be safely substituted for the simple ether anesthesia, giving the ether by a simple cone.

Dr. G. de N. Hough, New Bedford: I should like to emphasize the importance of the gas-oxygen anesthesia as

an educator of the surgeon. It is quite impossible for a surgeon to have his patients do well with gas-oxygen unless he adjusts himself to the conditions. With gas-oxygen you must manipulate with care and delicacy, there is no place for the rough surgeon with gas-oxygen anesthesia.

DR. ALLEN: Dr. Mahoney spoke of anesthetics to be used in the treatment of alcoholics—I would like to say that my treatment of alcoholics is to give large doses of morphine and to use the chloroform element in inducing anesthesia. What they require is a strong drug like chloroform. I use C. E. mixture or anesotal to begin with and then they stay well anesthetized for the whole operation.

What interests me about this meeting is to get at the true sentiment in regard to gas-oxygen. I will again read what Dr. Bennett said about the matter and try to arrive at what the true status of the situation is. (Read Dr. Bennett's statement.)

Now I have had one death myself, and know of three others that have occurred in Boston, and that shows that gas-oxygen is not really so very safe, and there must be something wrong. I have been thinking, while sitting here, that possibly the solution of the problem is in having a perfectly steady flow of gas and a steady flow of oxygen; you may in that way minimize the dangers of gas-oxygen anesthesia. My case was anesthetized by the Hewitt or the Teter apparatus, and I have seen alarming symptoms occur with both methods. The thought has occurred to me in administering it that if gas-oxygen is going to become the coming anesthetic it is going to be by having a steady flow of gas and of oxygen, and by preliminary medication of the patients by atropin or morphine. I would like to know what the gentlemen think about the safety of it.

M. C. SMITH, D.D.S.: After twenty-five years' experience with nitrous oxide I consider it perfectly safe. In those cases where complicated apparatus is used there is a large element of danger in something going wrong with the apparatus; the simpler the apparatus that can be used the better.

My experience has been that following nitrous oxide with ether is the ideal anesthesia. There should be great care

taken in selecting cases as some patients do badly under nitrous oxide. Patients who should not be anesthetized by this method are—

1. Alcoholics.
2. Persons who have been inmates of insane asylums who invariably do badly under nitrous oxide.
3. People in certain employments.

We had one class of patients in our city who worked on pasting lining in shoe shops with a rubber cement which had been dissolved with gasoline, these people were intoxicated with gasoline, and if one of these patients wanted nitrous oxide you were bound to have trouble every time, and anyone unacquainted with nitrous oxide would consider the symptoms very alarming.

A chauffeur is not a fit subject for nitrous oxide or any other person who handles large quantities of gasoline, or any person who may be partly intoxicated with any of the light Hydro-carbon oils.

DR. MANDELL: The subject of the safety of gas-oxygen anesthesia is one that has appealed to me. I knew before I went out to Cleveland to Crile's clinic the statement made by Dr. Bennett and I hesitated to look the subject up, but after going out there and hearing some of Crile's statements I went ahead.

The important thing, it seems to me, in regard to gas-oxygen is the personal equation of the man who gives it. I think that one must have had a large experience in anesthesia in order to be able to judge the fit patients. I cannot explain how I know that patients are in danger, but the expression of the patient, the look of the patient gives one warning. I am almost always fearful when I give gas-oxygen, and I watch the patient as carefully as I can. I have never had dangerous symptoms appear, although I am always on the lookout. I cannot add anything to the scientific side of the gas-oxygen question. I appreciate Dr. Bennett's statement and his opinion is of great value.

DR. F. J. COTTON: About the margin of safety in the nitrous oxide anesthesia. I do not want to be too positive but I think that the safety of the method depends upon three things:

1. A constant, steady flow of the gas and the oxygen.
2. Low tension of the gas; and
3. A careful watching of what we call the expiratory accent. I think that the record of that as a danger sign is important. If you are working under low tension with a steady current of air and know definitely the working values of each, I feel perfectly safe with it. Of course what Dr. Bennett says is to be considered very seriously, but I think that the work at Crile's clinic is a satisfactory test of the method; he is a very careful surgeon, and has had more experience than anyone else. I think that what I consider as safe, and probably unsafe, are well illustrated by the way that the Teter brothers give it, that is under high tension, and the way it is given at Crile's clinic. I think it is mainly a question of pressure and a careful watching of the patient.

DR. ALLEN: I must not be interpreted as condemning nitrous oxide, all I am trying to get at is—as Dr. Cotton has explained—what is to be done to increase the safety, and I think he has explained it; that is, the constant flow of both gas and oxygen and the use of low-pressure cylinders.

ARTICLE XII.

WHY AND WHEN TO OPERATE ON FRACTURES.

By F. J. COTTON, M.D.
OF BOSTON.

READ JUNE 13, 1911.

WHY and when to operate on fractures?

Why?—only because non-operative results are bad, very bad, in certain cases and classes.

When?—only when we can promise better results under operative measures.

This opens up the broad and important question, not of rules, but of circumstances, and I see no way for the present that the complex question can be answered save by a surgeon who has had a fair amount of experience with both methods, who really knows what can be done by skilfully applied, non-operative, "old-fashioned" methods, judged by the end-results (the only criterion worth bothering about), and who understands clearly that the present furor for "plating" fractures does not altogether lead to a bed of roses.

Any operator can plate a fresh fracture,—the question is,—had he better plate it or leave it alone?

I think I was one of the first in this community to do routine open operative work on fractures, eight years ago, at a time when few in Boston, except the late Drs. Porter and Burrell, used to touch any simple fracture (save the patella or olecranon)—a time when I had to get at least two seniors in consultation before I was allowed to touch a case of this sort.

Since then, my work has grown more radical, and my

field of operative work has broadened in this class of cases as I have tested results.

Within a short time a certain surgeon, not of maximum repute as to surgical judgment in other matters, has come here and declared that we should plate many fractures, if not all, and—lo! what has happened?

Everyone buys unto himself plates; everyone takes to plating.

Not only the difficult cases are subjected to operation, but fractures of the elbow, nay, even Colles' fractures are subjected to surgical incision and the insertion of indiscriminate hardware.

No one seems to have recalled the fact that the English treatment of fractures by splints, like that on the continent, was notoriously bad and sorely in need of improvement; no one seems to have questioned whether much of the American routine was not, after all, pretty satisfactory in trained and skilful hands; no, we must straightway "plate"!

In the face of this fresh flood of enthusiasm, I feel that I am a hopeless back-number.

I am not ready to say that all fractures should be operated on, nor to assent to the notion that all fractures not easily reducible call for open operation, nor yet to the notion that all fractures not perfectly reducible belong to the knife of the surgeon.

I have never seen a fracture with displacement perfectly reduced without incision, but I have seen many fractures that have been so excellently handled without operation that the results have left nothing to be desired.

After all, we all refer eyes to the man who knows eyes, though possibly it would be easier to have the general practitioner do the easy operation of enucleation rather than bother with the eye specialist, who might not operate.

Perhaps in fracture work it might work no harm to consider the man who knows fracture-work, rather than to rely on the general practitioner.

XUM

Just now, we are in the grip of what seems to me a foolish wave of enthusiasm for operation on fractures, and I am sure that in spite of any protest I may make much more radical operative work is going to be done here on simple fractures in the next five years than I would care to do.

I am going to present to-day what I believe (so far as it may be formulated) as to why and when one should operate on fractures.

To begin with the most obvious problem, there is one class in which we must intervene—the compound fractures. Here it is not a question of interference with the danger of introducing infection, but a question how one may best deal with a bone-wound already putatively infected. The chance of trouble is far greater than with a deliberate operative wound, but it is a chance we must face.

What shall we do? Certain compound fractures do well anyhow! Shall we trust to luck, with the chance of safe healing on one side, on the other the possibility not only of staphylococcus or streptococcus sepsis, but of gas bacillus or tetanus infection? The surgeon will take the surgical chance and will open up and clean up.

Save in cases in which age or infirmity, shock, or contrainsication to anesthesia force us to consider the vital rather than the local indication,—save for these cases we must intervene, and we must act radically in nearly all fresh compound fractures.

Compound fractures may be divided, roughly, into three classes*: First, the cases in which the fractures are merely incidental to a wholesale crushing of the soft parts—a condition not uncommonly met with in severe injuries to the hand and foot.

These cases are so largely involved in questions of tissue necrosis, in questions of vessel injury and nerve injury, that the treatment of the fractures as such is often

* I am not considering gun-shot fractures here.

negligeable. They hardly enter into the problem before us for consideration.

Broadly speaking, all we can usually do here is to clean up and secure decent position. In the past this has been a question of chlorinate of soda and soap, corrosive or alcohol dressings or dry dressings,—with such provision for blood drainage as may be needed. The results have not been particularly good in the past.

To-day, we have presented to us the scheme of sterilization with iodine tincture. It is too soon to talk of results with this method, save that they seem rather better than under the older routine.

One word only I would like to wedge in; namely, that iodine is an irritant as well as a disinfectant, in these cases as in the use of iodine for wound dressings and for sterilization of the operation field. I think I have done better with repeated applications of a 2% alcoholic solution than with single applications of the officinal tincture.

Apart from these cases of grave crushing injuries, compound fractures fall definitely into two classes (in both of which the broken bone has inflicted the damage), namely, those in which the prominent fragment of the broken bone has merely protruded to the skin level—has just "pinked" the skin, and has gone back, and those in which the bone has either torn the integument widely or has itself been driven out through the skin, and often is found greatly contaminated with all sorts of dirt.

It is only in the first class that we can hesitate. If the sharp corner of a broken tibia has made a bruised hole in the skin of perhaps one eighth inch across—as so often happens—it can hardly have come out far enough to gather much infection before its retreat into the tissues.

Everyone knows what a large proportion of these cases heal cleanly under any reasonable antiseptic treatment or even without it. Everyone knows that the "Treves"

treatment of fifteen years ago—air exposure with protection of the wound by aristol dusted on—gave good results.

But some cases showed and still show sepsis or even the dreaded gas-bacillus infection.

What shall we do as a routine?

I can only give my own answer.

If the case is in camp, in the country, or if the patient is in poor shape to do anything with, we may take a chance on external disinfection and internal immunity-reaction. Infection is of course present, but it may be so combatted in the tissues as to do no harm. It is justifiable to disinfect with corrosive, alcohol, or iodine, and take chances.

If, on the other hand, the best hospital facilities can be utilized, and if an experienced operator is at hand, I believe we are safer in opening up and radically cleansing the wound.

In this class of cases I have found that my house-surgeons at the hospital usually favored the "closed" method, and justified their choice by results. My own results have been better with the "open" treatment. So I assume that here (as so often happens in surgery) an intangible personal element enters in.

Under any method in this class of cases there will be a proportion of failures—a proportion will "go septic."

I have not seen, however, any fatal sepsis, or any case of the dreaded "gas-bacillus" infection, follow on a thorough open cleansing of the wound.

In the remaining class—the more open compound fractures—we must clean up.

Often enough the bone that has protruded is ground full of railroad cinders or factory smut or the smear of shops, or even the filth of stables.

First of all we must clean the bone. Bone has a low resistance to sepsis at best; traumatized bone is even less resistant.

Clean the bone-end with a brush, bite out the dirty surfaces with rongeurs; scrub clean or cut out all devitalized tissues in the soft parts, wash with weak corrosive sublimate, followed by profuse douching with normal salt solution, and we have at least done our best at cleansing.

If the wound was foul, pack it open with gauze; thus we may best combat possible infection with the dreaded "gas-bacillus," that is so deadly, but so dreads fresh air—a strict "anaerobe."

As a rule, of course, the attempt is made in compound fractures to reproduce conditions that would obtain had the fracture been a simple one.

A question of grave interest is how much one should do in relation to operative fixation at the time a compound fracture is cleaned up. My own practice is to do rather little. I have even gone as far as to let a compound fracture heal and cut down later to secure better position. This Lambotte has also done, and advises, and his advice is worth listening to!

Perhaps it is not usually necessary to go as far as this, but I do believe that plating at this time, or anything more than the insertion of a single suture of wire—(or better of kangaroo tendon) is about all we had better do at the outset.

Remember that a compound fracture is already infected: disinfect as we will, we cannot get it really sterile. Given a good chance it may (and usually does) heal by first intention, but every unnecessary manipulation, and most particularly every foreign body left in, is a hindrance and a menace to aseptic healing and must lessen the chance of a clean result.

In a certain few cases vessel-injuries determine the absolute necessity of operation. If an artery is torn there can be no question—it is operation or gangrene.

If the artery is only compressed the indication is nearly as absolute.

Venous hemorrhage gives a little more leeway, for sometimes hemorrhages absorb or are tapped successfully, but hematomata from rupture of the larger veins usually call for operation.

Here and there, generalized swelling associated with hematomata (especially in fractures of the leg) calls for slashing of skin and fascia to relieve general tension and so to prevent impending gangrene. This, however, is in no sense an operation on the fracture as such, and may not even open the field of the direct trauma.

Far commoner as an indication for operation is injury of the nerves.

This indication is not obsolute, for nerve damage may be done at the time of injury (as *e. g.* in musculo-spiral contusion with fractured humerus) that represents no continuing trauma.

Commonly, however, damage to the nerves is from continued tension.

Oftenest such lesions occur with shoulder-luxations overlooked or for other reasons left unreduced, or in cases of fracture luxation at the surgical neck with the head left luxated. In such cases operative relief of nerve tension, due to stretching of nerves over the humeral head, is insistently called for, irrespective of any possible improvement in bony relations.

The same remark holds as to elbow-luxations and fractures in which the ulnar nerve is damaged.

This latter injury is rare: I have seen but two cases, in both of which a separation of the epicondyle had accompanied a rotary luxation: in both the broken chip was displaced into the joint: in both, open operation resulted in cure of the nerve damage as well as in good elbow function.

All the foregoing indications for operation are substantially absolute!

Are there any uncomplicated fractures which should always be treated by open operation?

I know of but one,—fracture of the patella with separation of the fragments.

Even here we must qualify our absolute indication by the statement that non-operative treatment gives very tolerable results and is to be preferred in the aged or the infirm.

Nevertheless, nearly all patellar fractures should be sutured if we can command proper surroundings and proper skill.

What is the reason?

First, because the non-operative results are relatively poor.

Second, because operative treatment saves months of time!

I think this time element very important, not only here, but also in regard to all fractures in which non-operative treatment gives very slow results.

Most accidents occur to working-men, laborers and artisans.

In these classes, perhaps in all classes, work is a composite result of hunger and habit! Remove the element of habit, and hunger alone does not suffice!

With men of these types if they are laid off for six months they come to the end of their resources and become "bums" — of the park or of the free-lunch variety, and never become workers again.

If we can halve or quarter the period of convalescence nearly all of them will go back to work.

This, to my mind, is a most cogent argument for operation in many fractures, including particularly the fractures of the patella, and, only to a slightly less degree those of

olecranon, shoulder, etc.,—fractures in which ordinary apparatus-treatment gives poor or long-delayed results.

In patella and olecranon cases incision is almost obligatory if there are no contra-indications. Of lesions at the shoulder, fracture with luxation practically always means open operation:—and all compression fractures of the os calcis are to be subjected to the remodelling and re-impacting process which I originated and have so often described: they are not subjects for open operation, however, save in rare instances.

Now, with these four classes—patella, olecranon, shoulder luxation with fracture, fracture of the os calcis, we have finished the list of typical fractures in which operative measures may be called the usual and routine procedure.

I believe I am correct in saying that there are almost no fractures or luxations save those above noted in which a routine primary operation is justified.

In all other fractures I believe it is our duty to try non-operative treatment first.

Our danger to-day is that of neglecting skilled non-operative treatment, and leaning back on operative open reductions, just as we, many of us, on the advent of the x-ray neglected all that had gone before and abjectly referred everything to the radiographer for diagnosis.

To-day most good observers concede to experienced skill a value not less than that of an indifferently interpreted x-ray picture: and we are presently coming to learn, I think, that really skilful handling of simple fractures with apparatus is usually efficient.

Fortune has favored me with a considerable experience in operative fracture work, but a very small percentage of my cases have been cases that I saw immediately or shortly after the original injury, and of the rest very few cases indeed had gone through the hands of either surgeons or general practitioners whom I consider fully competent from

mechanical intuition or through clinical experience, to handle fractures to advantage.

There is no manner of question that there are many fractures that no one can reduce and hold properly. My only contention is that it is our duty to try to handle these cases without incision, and to operate only when we fail in this trial.

This is our duty to-day and will continue to be our duty until we can show better statistics as to the risks of operative treatment.

It does not help in the least that some one else says "— sepsis — never — never have it!" We know that we do have it, occasionally, — and it does not help us that another alleges that he does not.

To-day, in this country, in this community there is rarely any justification for open operation on fractures* unless and until really expert effort has been exhausted in the direction of non-operative treatment.

Our duty is to try the simple and safe methods first.

To test the results of such efforts we need only the "x-ray within a fortnight," which I have so often and so tiresomely championed. It may fatigue you very much to hear it, but I still believe we should try for the best reduction we can get, and then — within two weeks at most — get an x-ray— a good one — and interpret it in clinical terms and decide whether we should undertake more radical treatment by readjustment under ether or by open operation.

Perhaps it may be well to review in brief the more usual fractures and luxations in this sense — to consider what reasons should impel us, in the given case, to resort to operation.

Please remember — if I am right — that we are here dealing with injuries that call for operation only for some special reason.

* Except in the very limited classes above noted.

Commonest of all fractures is Colles' fracture at the wrist. When should we operate on these cases by open operation?

The answer is, almost never if the case comes to us fresh. In the worst cases, not readily held in place, the cause is a comminution not to be remedied by operation.

In unrecognized cases that have gone for some time, or in cases in which (perhaps without fault on anyone's part) the result is not very good, we can operate,—easily and successfully! but it is to be remembered that in operations on Colles' fractures, especially in operations on old consolidated fractures, united but united with malposition, the results of the operation are essentially cosmetic. The wrist will look better, but probably will not work any better than if we let it alone.

Fractures and luxations of or in the carpus are a worse problem. Many of them call for operation. But if we have, for instance, a fracture of the scaphoid bone with displacement, or a scaphoid fracture with backward luxation of the distal row of carpal bones, we should in the first place try to reduce the displacement. Only if the displacement recurs, or if a bad result is evidently assured, are we justified in operating, for operation in these cases means excision of the carpus, total or partial — often a very useful operation but not to be undertaken lightly. At least half these cases, however, do eventually call for operation to get good function.

As to fractures of the forearm—of one bone or both—what are we to do with these?

We are first to recognize that they usually do well under non-operative treatment, skilfully carried out. Position of fragments so bad as to be striking in the x-ray plate is perfectly compatible with recovery of perfect function. Mutual overlapping of the bone-ends will not do to leave alone; but in even the most successful recoveries the position of one bone—the radius usually—is apt to be rather bad.

If we decide that the position is too bad to leave there is something to be said as to the manner of operation. All we need here is to bring bone-ends opposite one another—and there is no great displacing force to be counteracted. This can most securely be done with plates, but unfortunately plates in the forearm tend to make trouble, and I have found that staples or sutures could be so applied (with splints) as to give results, and have never had any trouble. Several of my forearm cases have been operated on to quicken delayed union, and in all these staple or suture fixation has been sufficiently firm to ensure the results wished.

Fractures of both bones just below the elbow, or radial luxation with ulnar fracture, may call for early operation if irreducible, but as a rule such lesions are readily reducible.

Olecranon fractures call for open operation as a rule, as noted.

Fractures of the radial head and neck call for operation very often, but not as a routine.

Many of these cases do very well anyhow under early motion and massage—even where the skiagraph shows a much deformed head of the radius. All we care about is the recovery of the motion of rotation.

The rule is to test at three to five weeks and see if motion is coming back—if not, and if the check to motion is bony—then excise the radial head. Few operations are more satisfactory in results than this—perfect recovery of function being the rule, but it is not needed as a routine.

Now as to fractures of the humerus at the elbow—a common fracture in the adult—one of the commonest in children. Should we operate on these cases as a rule?

I think not, as a rule, and my conclusions are based on examination of the end-results of a rather long series of cases.

The results of non-operative treatment are far from bad,

even in adults, if early massage and active motion are taken advantage of, and in children the results are good,—far better than one would think from reading the classic and pessimistic text-books on the subject, and we need some really good reason for subjecting the patient to the risk of operation.

Unless there is some peculiar difficulty in reducing or holding the fracture, or some injury to nerve or vessel trunks, I should not think of operating on a fresh case of fracture of the humerus at the elbow. When I have operated, I have not used plates or even staples and rarely even pins or sutures.

The late operations will be considered further on.

As to fractures of the humeral shaft the question is different!

Here we have a definite indication for interference in many cases—the humerus is a single bone—unsplinted, so to speak, by a sister-bone—and displacement is often great and hardly controllable, while the chance of long delayed union or non-union is relatively great.

No quarrel is to be made with the surgeon who proposes operative interference in most fractures of the humeral shaft. His results will probably show up better in the long run than non-operative handling can show as a rule. Personally, I do not operate often, but this is because I think I can do things with splints. In cases in which splint treatment is unsatisfactory, I have had no hesitation in carrying out the open treatment.

When we come to fractures of the "surgical neck" of the humerus, or the separation of the epiphysis, which is the youthful equivalent of this fracture, we face a special problem. Nearly always in these cases reduction without open operation is peculiarly difficult.

It should be tried, and in a fair proportion of cases it succeeds. If it does not, we must cut. These fractures

are hard to reduce, partly because soft tissues are often jammed in where they do not belong, but more particularly because the pectoral muscles adduct the lower fragment and the biceps draws it up. Perfect position is not essential, but the mechanics of the joint requires pretty good position if we are to get good function, and if need be we must operate to get it, and we must so operate in a large percentage of the cases.

The difficulty is one of reduction, not of retention.

We often must cut down to reduce the fracture, but after it is reduced we need nothing more than a suture, often not that; adduction of the arm maintained with adhesive plaster gives admirable results in many cases; if there is rotation of the head outward, abduction may work better; in any case we need adjusted position of fragments and fixation of the arm, not any special retention apparatus buried in the wound.

Fractures of the anatomical neck are usually best let alone. They come out pretty well, even in cases with a loose unimpacted head, and if we operate we shall have to take out the head anyway.

The cases (two in number) that I have operated on have been fractures complicated with luxation; in both of them the head had to be removed.

All these cases of luxation with fracture, whether the fracture is of surgical or of anatomical neck, should be operated on, particularly as they are so often complicated with problems of tension of the brachial plexus over the displaced head, etc.

As to pure luxation of the shoulder there may be a little leeway for judgment.

In fresh luxations we may say that practically all cases are reducible by manipulation. I have failed once, but this was in a case of an out-patient who refused ether and disappeared.

A certain number of cases came to us, however, usually unrecognized, at two weeks—two months after injury. These cases should be operated on, in my opinion, by incision, unless reduction is really easy. No one who has operated on these cases and seen how massive the formation of new tissue is, and how vessels and nerves are bound down to the displaced head, will ever feel safe in applying much force to such a case in trying reduction.

The results of operative reduction in these luxations are not by any means perfect, for some of the short rotators are sacrificed usually, but the function is far better than if the joint is left luxated.

However, it is to be said that such operations are bloody and carry much shock, and that in old infirm people it may be justifiable to leave the luxation if it is not giving pain, for the functional results in such cases may in time be very fair.

I find that I have operated on sixteen cases of shoulder injury.

Four of these were luxations, pure and simple. Three were fracture luxations. Seven were fractures of the surgical or anatomical neck with malposition.

In three, the involvement of nerves by pressure was an indication for interference, and there was one case of rupture of the artery, one of the vein; both treated successfully.

Clavicle fractures do not call for operation—ever, as far as I know.

Clavicle luxation at the acromial joint, on the other hand, is distinctly operable. Only by operation do we get anything like perfect results. The operation is simple, and the only reason it is not regularly done is that the lesion is not a particularly painful one, and is likely to happen in laborers who care little about the treatment of anything that doesn't hurt them, and are indifferent to the weakness resulting from the ordinary treatment.

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Next in order come hip-fractures, often operated on, but in my opinion ordinarily inoperable, unless luxation is combined with the fracture. Most hip-fractures are impacted: if impacted in a tolerably good position it is a crime to disturb the impaction. If position is very bad (*e. g.* if outward rotation is extreme) we may in selected cases break up the impaction and correct the position and then reimpact, (I have done this successfully in one recent case) or we may put the limb up in abduction after breaking up impaction.

Union usually occurs even in cases originally unimpacted if we use abduction-treatment, and cases recently reported by me show the feasibility of artificial impaction for added security.

Only cases in which treatment by apparatus has been tried and has failed justify open operation, and the proportion of these cases after all is small, if we except those occurring in old enfeebled individuals in whom operative treatment of any sort, for any cause, is not to be considered. I have personally done such fixation operation but four times, always with improvement in function, but never with anything near a perfect result, and never could I convince myself of any bony union. From these cases and those of others the operation strikes me as useful, but not anything to get enthusiastic over.

Fractures of the femoral shaft will be considered later, with your permission. Here I will only say that I believe very many of them to be operable, and think operations far too seldom done.

At the knee, patellar fractures are nearly always operable, for reasons already given.

Separations of the lower epiphysis of the femur unless easily reducible are always operable on account of the peculiar danger they present of vessel pressure and consequent gangrene of the leg. Rarely, fractures near the knee, of the femur or of the tibia, may call for prompt interference for the same reason.

Fractures of the lower leg, responsible for much disability in very many cases, call for better treatment than they usually get. Better treatment than they usually get is usually practicable without incision, and in the worst cases, those with comminution, open operation is to my mind out of the question.

On the other hand many cases do admirably that show up badly in the x-ray. So far as may be formulated we may say that any fracture so ill-reduced that the fracture-surfaces are not at all in contact is too bad to let go by. Many other cases should be operated on, but at least these must be fixed, unless there are good reasons to the contrary, and even this rule would mean many more operations and less cripples than at present.

As to ankle fractures the question is different: here it is a question of reduction, not of holding the position: once reduced these cases are easily held (with the rarest exceptions) and if these cases come to us fresh they will furnish practically no open operations: Personally I have never operated on a fresh ankle fracture that was not compound and have not regretted my abstinence yet.

The same applies to fractures of the tarsus and foot, save for the os calcis fractures, which almost uniformly call for reduction and reimpaction, but not for incision. Even if we wished to incise and reduce in these cases the risk of sepsis in opening down through thick doubtful skin on the always comminuted mess of cancellated bone is almost prohibitive.

This is but a rough incomplete survey: in general it is a plea for operative measures restricted to cases in which there is a definite reason for operating on the individual case.

In general I am inclined to fear too great an activity in fracture operating in the next few years.

There are, however, two classes of fractures, by no means

always to be operated on, that nevertheless deserve frequent operation because operation is often the only way to get good results, and because the ordinary results are far worse than the profession realizes; namely, fractures of the femoral shaft, and leg fractures. The accepted theory is that these cases usually do well. They very usually do not, if one is at all careful as to what he calls doing well.

Only those who see the shortened legs, the occasional vicious deformities, the routine stiff knees, the slow unions, the common refractures following our treatment of femur fractures can appreciate how much is left to be desired, and in leg-fractures the condition is only a grade better. Now these lesions occur in young working-men as a rule who need good legs to work on, and I think no one who has looked much into the matter of end-results will hesitate long between the probability of crippling and the chance of operation in such cases as cannot be well reduced and held: for to-day what between "plating" and such screw-bolt apparatus as I have repeatedly used, we can in any case be sure of reduction and of maintenance of practically perfect position, and (bar sepsis) of a greatly lessened period of convalescence as well as of far better end-results.

Generalizations are notoriously unsafe,—perhaps unwise, but I will hazard a guess that about half the femur-fractures as they run (including nearly all the transverse fractures) and of the leg fractures not less than a quarter of the total, will in the future be considered operable if perfectly satisfactory adjustment is not brought about within ten days.

I have not spoken of late operations—on fractures badly healed, and this paper is perhaps no place to consider this subject, in itself already grown to be a large one. Suffice it to say that, particularly in fractures about joints wonderfully satisfactory work can be done. Particularly is this time of injuries of the carpus, of the elbow and about the ankle. I think I can truthfully say that I have never had or seen an

old fracture in any part subjected to proper operative measures that was not improved thereby, and in many cases the restoration of function is unbelievably good, even if a long time has elapsed since injury. These are, however, complicated special operations as a rule, and we have no time for their discussion now.

Let me say only that I believe late operations on cases showing disability after fracture offer a field second to none in usefulness, probably even more important in the present stage of fracture-treatment than open operations on fresh cases.

They are, however, incomparably more difficult than the operations on fresh cases and are successful only in proportion to the care expended in planning the operation suited to the individual cases and to the perfection of the operative technique.

To return to the operative handling of recent fractures—a brief summation is in order:

We have,

(1) Operable as a routine.
- *a* Compound fractures.
- *b* Fractures complicated with vessel-lesions.
- *c* Fractures complicated with nerve-lesions.
- *d* Patella fractures.
- *e* Olecranon fractures.
- *f* Fracture-luxations, especially at the shoulder.
- *g* Acromio-clavicular luxations.

(2) Usually operable after trial of milder methods.
- *a* Carpal injuries.
- *b* Fractures of the femoral shaft.

(3) Often operable.
- *a* Humerus fractures.
- *b* Leg fractures.

(4) Operable on occasion: All fractures in fit patients, that promise a bad result as they lie, even though they have

had good care, provided, that we can show good reasons for expecting a better result from operation.

In general, we may say that fractures of the wrist, forearm, hip, knee, ankle, and foot, call for better non-operative treatment than they get to-day; that fractures of carpus, humerus, shoulder, shaft of femur, and lower leg, call for more and better operative work than we have given them.

Technique deserves only a word.

In large measure technique is, after all, personal and non-transferable. In this work we most need

a A clear idea of what is to be done.

b A clear field.

c A minimum of cutting.

d A minimum of handling.

e Accurate reposition.

f Adequate fixation, with a minimum of foreign material left in the wound.

We must use adequate means for fixation; but, in general, I think accurate reposition of fragments is usually the main thing. Beyond this, muscle tension does much; the enclosing plaster may be made to do more, and in the end we come out very well, usually, if we have inserted only a suture—perhaps a staple. Often enough we need neither.

Where there is a pull of strong muscles, as in fractures of humerus, or lower leg, and particularly in femur fractures, the problem is different. We must have something more rigid. Here is where the bone plates come in, and as far as I can see about the only place they come in legitimately. In cases of femur fracture, I have known even bone plates torn out under the tension. Under these circumstances a fixing apparatus with removable long bone screws is better even than a plate.

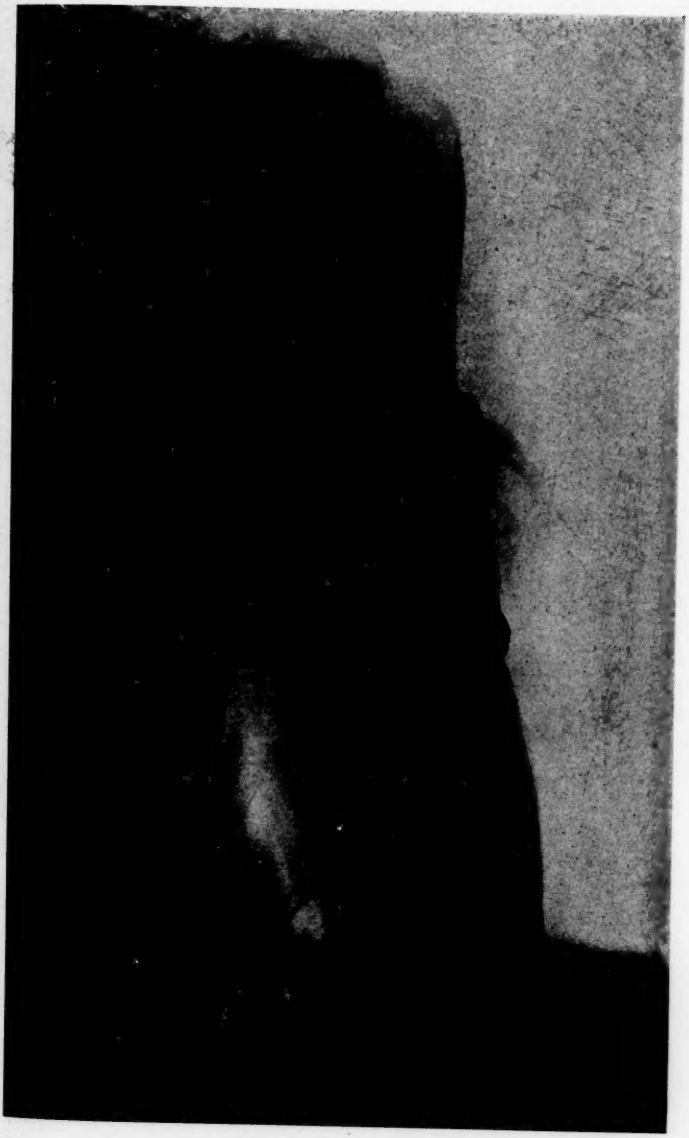
ARTICLE XIII.

EXPERIENCES OF A BEGINNER IN THE USE OF LANE'S PLATES IN FRACTURES.

By F. B. LUND, M.D.
OF BOSTON.

READ JUNE 13, 1911.

The progress of surgery in many different departments during the last fifteen or twenty years has been of almost unexampled rapidity and interest. The surgery of the pelvis, kidneys, ureters, the stomach and the intestines; the brain, nerves and nervous system have all presented fascinating fields for the surgeon's efforts. The literature on these subjects has been enormous, and for one who is an active worker in surgery to keep pace with the work that has been done in various parts of the work and to keep up with the rest of the procession has indeed been a difficult and impossible task. The interest of our work, however, and the enthusiasm of the workers has been such that wonders have been accomplished by many surgeons throughout our country and, indeed, throughout the world. While the work in these comparatively new and fascinating fields has absorbed most of our interest, we have still had with us the old, old cases which surgeons have always had in their care, none of which have been more in evidence since men began to live upon the earth and suffer from accidents, than fractures and dislocations. In a large surgical service in the City Hospital, the fractures and other traumatisms supply, perhaps, one-third of the cases. They have been, and are, tedious and difficult to treat. The re-



CASE 4. AFTER OPERATION. FIG. 1.

sults attained even with the utmost care are frequently far from satisfactory, especially since we have had the x-ray to show us how badly an x-ray picture may make a fracture look in which really the functional result is fair. A certain amount of overlapping and shortening has been expected, for instance, in fractures of the femur, and in one corrected by a high sole a shortening of an inch or so does not materially interfere with function though a slight limp may result.

Certain fractures of the femur, however, have resulted in long delayed union, and finally in union with deformity, marked overlapping and bowing. Certain spiral fractures of the tibia have resulted in bowing and shortening, and fractures in the vicinity of joints, such as Pott's fracture, have resulted, even where considerable care is given, in eversion and pronation of the foot and considerable disability. Fractures of the humerus sometimes gives us non-union and, in my experience, non-union has not been rare in fractures of the middle of the forearm.

The long, tedious treatment of many fractures and the ultimate unsatisfactory results, have been such that in a large and varied hospital service some of us have felt that especial attention to fractures was, perhaps, not worth while, as the results did not justify the effort expended. The brilliant and fascinating work which we could do in other fields has so attracted our interest that possibly we have to a certain extent neglected the fractures. I, myself, plead guilty to this. In spite of the fact that with deformity we may still have very fair functional results, certainly deformity does not help functional results, and in some cases we certainly get non-union with false joint, mal-union with marked shortening and even mal-union with deformity and marked disability. Occasionally, such fractures have been operated upon. They have almost always been old fractures with non-union or union with deformity.

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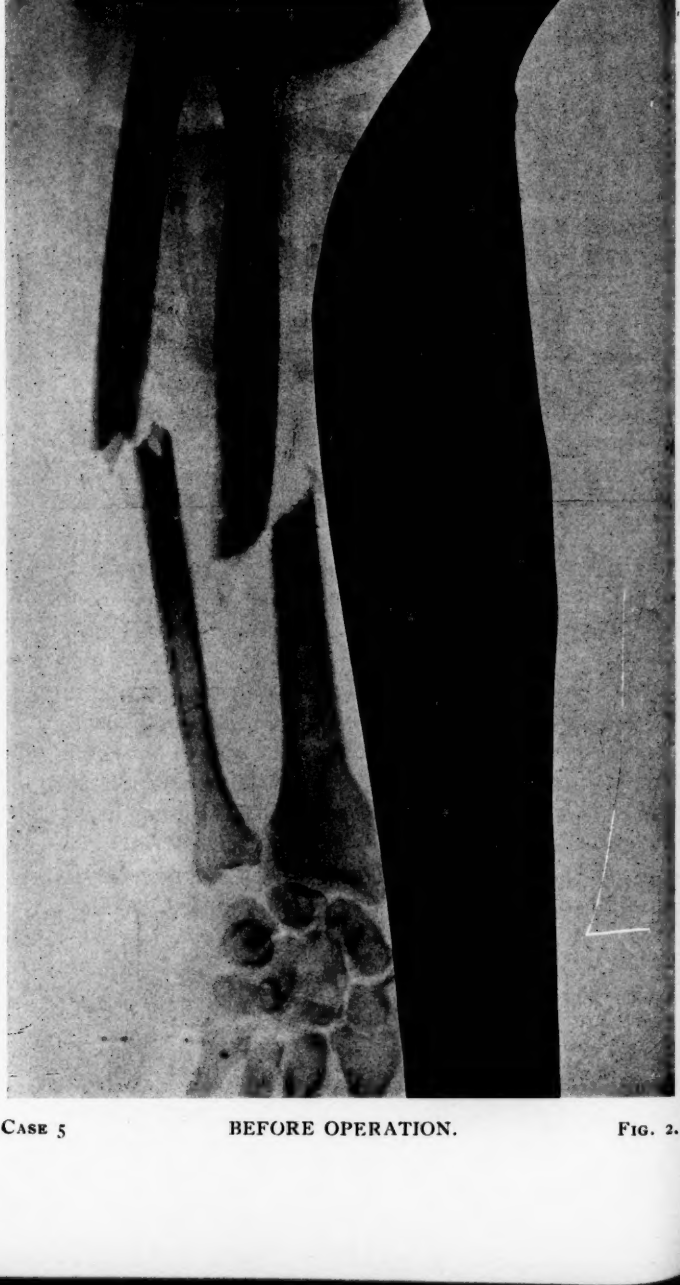
The operations have been difficult, have consisted in cutting down upon, freshening and sometimes sawing off the ends of the bones, getting them together by a firm extension and retaining by suturing with heavy wire of some kind or by kangaroo tendon or coarse chromic gut. In operating on such cases as these, I have, myself, found that after I had got the bones in apposition, and had sutured them and was applying a retaining plaster of Paris bandage that I was filled with anxiety lest the ends should slip apart even from the manipulations necessary to applying the bandage, so flimsy and unsatisfactory did the attachment seem which was made by wire or by suture. The operations have been difficult and serious, and while usually efficient in securing union, an x-ray has sometimes shown that the bones have slipped apart and that the wire had either broken or so stretched out of shape that the apposition was far from ideal.

Tendon has been used whenever possible, and wire only rarely, owing to our fear to leave a foreign body in the deep tissues and our knowledge that occasionally we had to take out the wire. Personally, I have used wire and, occasionally, nails, screws and staples, but only in cases where I found that suture of the soft parts about the bone was inefficient. This was in a fairly large portion of the cases. Fresh, simple fractures, no matter how much overlapped and comminuted, in our hospital, we have very seldom operated upon, because we believed that the methods of extension and the application of splints, while not giving ideal results from the x-ray point of view, would give sufficiently good functional results, and as operation was so difficult, there was danger of infection, and the use of wires and screws was so uncertain that it was not worth while to attempt it.

In other parts of the world, however, opinion has not been the same. Simple fractures which were badly over-



CASE 5 BEFORE OPERATION. FIG. 2.

CASE 5. AFTER OPERATION. FIG. 3.

lapped and which the x-ray showed had not been sufficiently well reduced, have been treated by operation by the open method. The bones have been cut down upon, brought together by firm extension under the guidance of the eye, and a metal plate with screws fixed to the shaft of the bone to hold it in position. These plates have been left in. Certain surgeons, fearing to leave in the tissues so large a foreign body, have devised apparatus by which long screws were screwed into the fragments, the ends projecting through the wound and attached to an external metal or wooden splint, such as the clamp of Parkhill or the wooden splint of Cotton. These pieces of apparatus, while sometimes efficient, were cumbersome and difficult to apply, and as they projected through the open wound during the necessarily prolonged course of treatment, there was some danger of infection taking place, with most unsatisfactory results. Other workers in this field have applied metal plates to the bone itself, some stiff and rigid, others pliable and capable of being attached to the surface of the bone. Sick's plates are an example of these. These flexible plates have been much used with advantage in resections of the joints, such as the knee, retaining the bones in apposition so that union is much more rapid than if one depends simply upon splints for fixation.

In regard to the operative treatment of fractures, W. Arbuthnot Lane, of London, has been one of the most progressive workers. He has pointed out the frequent unsatisfactory results in spiral fractures in the long bones and fractures with a lateral dislocation near the joint, etc., and has felt, and I think justly, that some of our results which we have considered fairly good, are, perhaps, after all, a serious handicap to the active class of laboring men who are most frequently the subject of fractures in hospitals. He has devised a set of stiff, steel plates and a set of instruments for handling the bones. These plates, especially

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those for the femur, are so large and heavy that many surgeons, including myself, have for many years felt unwilling to use them on account of the fear of leaving in so large a foreign body. Even if infection did not take place at the time of the operation, which the difficulty and complication of the operation would certainly make possible, it was felt that this large foreign body might irritate the bone so as to cause a locus minoris resistentiae, a subsequent abscess and osteomyelitis. Lane, however, claimed that these plates healed in, and published a large number of photographs and diagrams showing remarkable anatomical results of the application of these plates to bad spiral fractures and fractures of the femur and humerus with overlapping. He maintains that the larger the foreign body one puts in the more careful one must be in the matter of asepsis. To secure asepsis he thinks it is of the utmost importance that the skin of the patient should be covered so that bacteria may not be carried from the skin into the wound, and that the wound should not be touched by the hand of the surgeon even while encased in rubber gloves, or a part of an instrument which has touched the gloved hand of the surgeon. He points out what is very true, that in manipulating these sharp, fractured bones with hands covered with rubber gloves, the gloves are frequently punctured, and infection may take place. With his long handled, periosteal elevators, drills and other instruments, I saw him last summer operate on a fresh oblique fracture of the femur, get it in perfect apposition, apply a plate which held it perfectly, and close the wound without once touching the skin of the patient or the wound with his gloved hands. It was a remarkable achievement, and one which I so far have been unable to equal. I would say, however, that all the cases on which I have had an opportunity to operate at the City Hospital this winter have been old cases, either of non-union, partial union with deformity or complete union

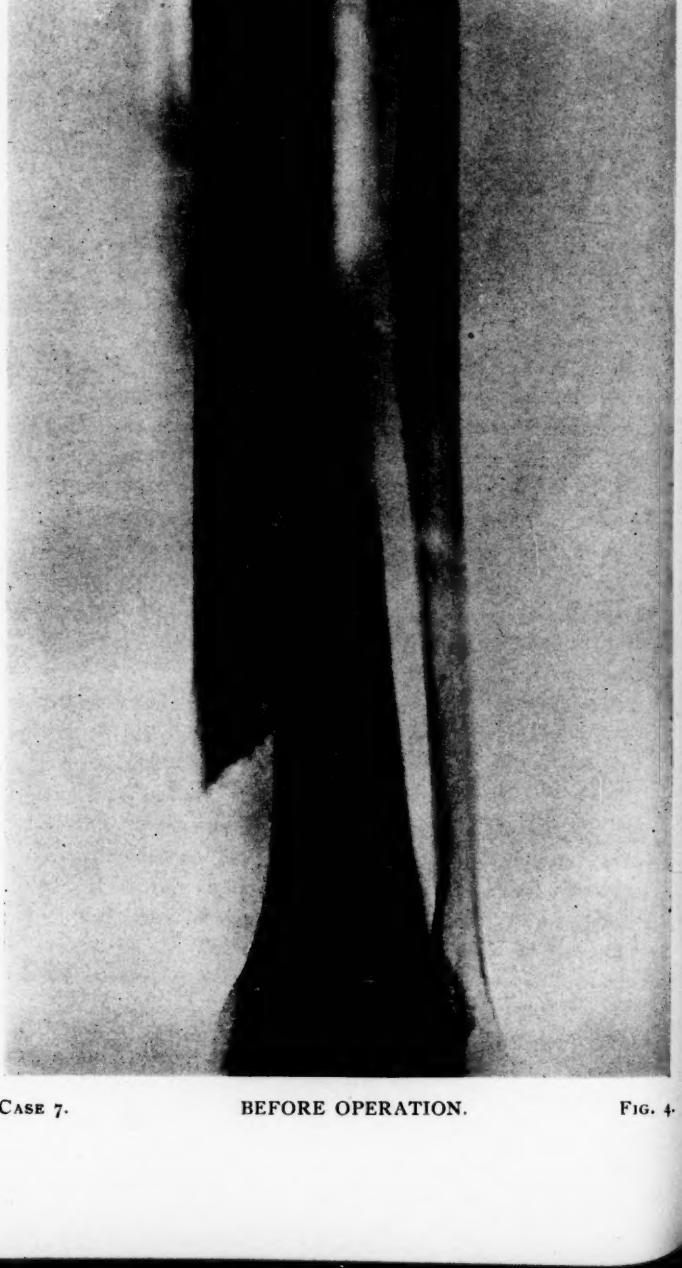
with deformity. In these cases, the ends of the bones are surrounded with a rough, partially calcified periosteum, called callus; the muscles are atrophied, contracted and adherent to the fracture, and the difficulty of clearing them away and getting the ends of the bones together is much greater than is the case in fractures operated on soon after the accident. I have, personally, found these operations difficult to perform, tedious and sometimes unsuccessful, but in the majority of the cases I have certainly been able to accomplish a good deal, to diminish or even entirely do away with the deformity, to get the ends of the bone in apposition and hold them by an internal splint to such an extent that the external apparatus, which was cumbersome, painful and, in fact, in one case unbearable to the patient, could be done away with and replaced by light, simple apparatus, and to secure union in cases which had remained ununited for from four to six months.

Four of my cases have been fractures of the femur, all of them old cases of non-union after long periods and, in two of them, the result was entirely gratifying both as to union and position. In two cases, the plates did not hold, although union has, I believe, resulted and the plates are still in. My ill success in these two cases has been due partly to the extreme difficulty of getting the ends of the bones into perfect apposition, partly to my lack of knowledge that the screws I used would not hold in callus, and largely to the fact that both of the patients were impossible to keep anywhere near quiet. The screws might have been reinforced by wire placed around the plate and around the bone. In the Pennsylvania Hospital screws have been used which went clear through the bone and bit into the cortex on the opposite side. These I might have used with success, or success might have attended a more efficient apparatus applied externally after putting in the plates. Dr. Richard F. Harte of Philadelphia, in an extremely interesting re-

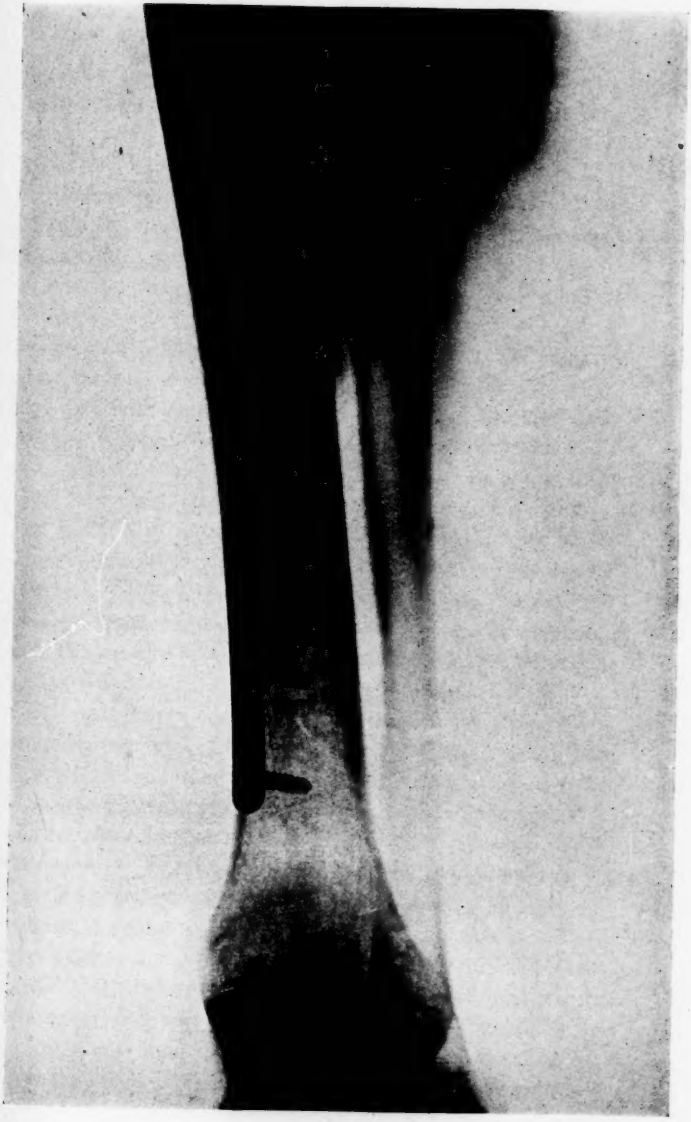
cent article on fractures of the femur, takes the ground that almost all simple fractures of the femur ought to be operated upon.

In the eleven cases upon which I have operated, I have had only one case of infection, and that was in the fracture of both bones of the forearm in which the plates had to be removed on the tenth day. After removal of the plates and drainage of the wounds there was a mild infection in which the course was afebrile for a week. Union in very good position, however, was secured. In another fracture of the forearm, I had to remove the ulnar plate at the end of three weeks on account of suppuration, but there was at no time any temperature. The wound firmly healed, and the result of the case was that I attained union without deformity in a fracture of the forearm which was ununited and badly bowed four months after the injury. In this case, I used an unnecessarily heavy plate upon the ulna and its position made it lie almost subcutaneously. I believe that one will rarely have to remove a plate which is applied to the humerus or femur, lying, as it does, deeply placed beneath layers of muscle which cover it easily and without tension. Plates applied to the tibia are usually put on the anterior surface, the skin has to be stretched over them and it is more difficult to get them to heal in. Of the three tibias which I plated I had to remove the plate in one. In this case, however, the removal was simple and was not due to infection, as the course was absolutely afebrile. A small sinus discharging serum led down to the plate and required its removal at the end of three weeks. The anatomical and clinical result of this case, of which I will show you plates, was perfect. The tibia is very easy to plate as the bones can easily be held in position while the screw holes are drilled and the plate applied.

This is not the case with the radius and ulna, the plating of which I have found very difficult and tedious, and the



CASE 7. BEFORE OPERATION. FIG. 4.



CASE 7. AFTER OPERATION. FIG. 5.

femur and humerus also I have found hard to hold while making the drill holes. I can show you my results in no way better than by recounting the cases and showing the x-rays which go with them.

My first case was a woman of sixty years with a very oblique fracture of the upper end of the femur, of which you see the x-ray here. There was a shortening of 2½″ or 3″, marked eversion and no union at the end of four months. A long external excision was made, the ends of the bones freed from rough irregular callus, and the contracted muscles about the upper end of the shaft, especially of the inner side, separated with considerable force and a great deal of difficulty. The bone was then pulled down into very fair position until there was only about ½″ of shortening, and fixed by a long steel plate with four screws in each fragment. The result was union with excellent position, very slight shortening and no eversion. The operation was done last October. The second picture, taken in January, shows the bones in contact and very good position. She was beginning to walk on the leg. The only apparatus used after the plating was a long external splint and posterior splint.

My second case was a spiral fracture of the femur with comminution, which, in the ward at the City Hospital, had remained absolutely ununited after three months. The x-ray will show you the condition, except the lack of union. It was cut down upon, and by very violent extension, after clearing away the callus, the bones were brought down in good position and a four screw plate applied. This second x-ray shows the position of the bone with the plate in. Very little apparatus was required to hold the femur, and rapid union took place. About three months later, however, a certain swelling and edema appeared on the outer side of the thigh and I cut down upon the plate. I found it somewhat loose and surrounded by broken down blood clot. It was easily removed, and the bone found in excellent position and firmly united. He has since done well.

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My third case shows, in this picture, a very poor result with non-union four months after the fracture of the left femur. It occurred in a young working man whose right femur was fractured in some accident, but which united well. In this case, I cut down upon the bone which, as you see, is surrounded by irregular masses of callus, cleared away part of the callus and got the bones into fair apposition. I was unable, however, to get them into perfect apposition, and believe that when my screws were inserted those in the lower fragment were drilled largely into callus instead of into bone. The operation was difficult and tedious, and a few days after its performance the man became very unruly and difficult to keep quiet, and the screws pulled out of the lower fragment, and the deformity came back almost as bad as before the operation. He remained in the hospital six weeks after that, but I was unable to get him to let me try again. If he had done so, I would have applied two plates instead of one, and believe that I could have succeeded. He left the hospital, however, with some union and they have not heard from him since. The plate was still in when he left.

My fourth case was a transverse fracture of the humerus in a woman of sixty, in which we made three attempts to get the bone in apposition, and the result shown in the picture was the best we could accomplish. As will be seen, the lower end of the humerus was carried upward and inward and overlapped. It evidently pressed on the main nerve trunks, as the old lady was very uncomfortable and in constant pain, and complained bitterly of the cumbersome apparatus which she had to wear. With difficulty we obtained her reluctant consent to an operation and cut down upon it, got the bones in excellent apposition and applied a four screw plate. After this, all that was necessary to hold the bones in apposition was a swathe around the body and arm in the day time and at night a shoulder cap to prevent

her rolling over and injuring it. The wound healed by first intention and the plate stayed in, and she left the hospital very much pleased with the result of operative interference.

My fifth case was a stout woman of forty who had fractured both bones of the forearm four months before and had failed to get union. The arm was, of course, helpless, and presented marked posterior bowing. It was cut down upon and a strand of muscle found between the ends of the ulna which was working between two sharp points like a rope over a pulley block. It was removed, the ends brought into apposition and plated with a radius plate; the ulna with a small femur plate. The healing was afebrile, but at the end of three weeks there was a fluctuating swelling over the ulna, and I was obliged to cut down and remove the ulnar plate. The wound healed promptly, and at the end of two months we obtained the result shown in the picture, which shows good union with no, or very slight, deformity, and a useful arm.

My sixth case was a fracture of the radius and ulna, of which pictures are shown before and after plating. The plates had to be removed in ten days, but with some care the wound healed and good union took place.

My seventh case was a spiral fracture of the tibia with marked deformity and projection of the lower end of the upper fragment under the skin. This was operated upon about three weeks after the injury, and by strong extension and with the use of Lane's strong bone holding forceps, the ends were brought into perfect position and held by a plate. The plate had to be removed after three weeks and, as as stated above, the anatomical and functional result was perfect.

My eighth case is one of union with deformity in a fracture of both bones of the leg. The picture shows the deformity, which was the best result that could be obtained in

this case without operation, by one of our surgeons at the hospital. Two months after the operation, he was etherized, the bone cut down upon, straightened, and then the plate applied. The plate remained in, and the anatomical and functional results were excellent.

My ninth case was a fracture of the lower two-thirds of the femur with non-union after four months. The x-ray shows comminution, a piece having slipped out over the side of the bone, the ends of the bone apparently in fair position. I cut down upon it, however, and found the upper end of the lower fragment opened out in a cup shape and a definite cyst between the end of the bone surrounded with newly formed membrane and filled with serum. The cyst and its cartilaginous lining were cut and scraped away from the ends of the bones and the bones brought into very fair apposition, and fixed by a plate. Some of the screws, however, I think went into the callus instead of into the bone shaft, and there was a strong tendency towards inward displacement of the lower fragment, which was, with difficulty, overcome. The operation was tedious in the extreme, but the man was put to bed with a stiff leg at least. About three weeks afterwards, while turning in bed, he felt something give way, and I saw him four weeks after that when an x-ray had been taken, showing that the plate had pulled out the two lower fragments and the bones had overlapped two and one half inches. The x-ray looked pretty bad, but union was evidently beginning to take place and there was no cyst between the ends of the bones. The leg, although shortened by one and a half inches, was perfectly straight and without eversion. It was thought, therefore, better to leave him as he was rather than operate again, and he was left in that condition.

My tenth case I will report here as we were ready to use plates but it seemed to me a case which would be better held by a staple. The case was a fracture of the sur-

CASE 8. BEFORE OPERATION. FIG. 6.

CASE 8. AFTER OPERATION. FIG. 7.

gical neck of the humerus, the upper end of the lower fragment being well up into the inner side so as to simulate a dislocation. It occurred in a boy of sixteen years. The fracture was three weeks old and had been, when first examined, mistaken for dislocation. A vertical incision was made through the anterior portion of the deltoid, the site of the fracture exposed and with very great difficulty the ends of the bones cleared, pried apart and the shaft of the bone brought into proper line. Although the two fractured surfaces came together well, there was a great tendency for the shaft to be pulled inward by the action of the pectoralis major. I had to saw about $\frac{3}{8}$" off the upper end of the lower fragment. The upper fragment, seeming to me too small to hold a bone plate, I drove in a large staple. The bone remainded in good position and excellent functional results were obtained although no x-rays have been taken. At the present time, June 6th, he has firm union.

The eleventh case was a case of spiral fracture of the tibia with marked overlapping in which, apparently, reduction was obtained and held by a plate. The picture of case 7 will perfectly well answer for the x-ray of this case. The operation was done ten days ago, so it is too early to speak of the ultimate results.

I believe that the staple instead of the bone plate may be used for a great many of these fractures and has the advantage that the amount of foreign material used is considerably smaller. I know that it is used very frequently by my colleague, Dr. Cotton. I think it is especially valuable in separations of the epiphysis or fractures near joints.

In three of the cases, of which there were four, where I have had to remove the plates, I am convinced that infection did not take place. Lane calls attention to the necessity of not putting ligatures on vessels, but of leaving pressure forceps on during the operation to control the hemorrhages. In two of my cases in which the plate had to be

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removed, one from the ulna and one from the tibia, I think the tension on the soft parts accounts for the irritation which made it necessary to remove the plates. In the case of the femur, I think that the hemorrhage had not been entirely stopped, that some hemorrhage took place, and a hematoma formed which may possibly have been infected afterwards through the blood stream. In none of the cases was there any infective osteomyelitis or necrosis. This is shown, in cases where the plates had to be removed, by the firm and permanent healing of the wounds after their removal. I do not know of a much worse plight for a patient than to be the possessor of an ununited fracture or a fracture with marked deformity and disability. If this can be prevented in a fair proportion of cases by the use of these bone plates, I think that it is well worth while. My results so far have been those of a beginner and the cases have been unfavorable for operations, for the reason that all but one of them have been old fractures with non-union or partial union with deformity with more or less callus about the ends of the bones. I believe that in the future when an x-ray shows us that a recent fracture of a long bone is badly overlapped, or in poor position, or that the ends of the bone are presumably separated by soft parts, that we will cut down upon them and reduce them by operative methods, provided we have the patients in hospitals where the aseptic technique is good, and where the surgeon has taken the pains to master the details of the not inconsiderable operation of the application of plates and screws to these fractures. Of course, in case the ends will spring together to stay without plates and screws they need not be used, but the use of plates and screws certainly very much simplifies the putting on of the retentive apparatus. I believe that as a result of this our ultimate results in fractures will be considerably improved, and this will certainly be worth a little extra labor on our part. I do not believe the danger of infection is

sufficient to deter us from attempting operation in fractures which are going to do better for it. If the results in old, ununited fractures are fairly satisfactory, how much better will they be in fresh fractures which we found we could not reduce satisfactorily.

I am far from advocating operation in all simple fractures, as very many of them, such as Colles' fracture and many fractures about the ankle joint, are easily reduced and tend to stay so after once reduction has been affected; but I do believe that in bad oblique fractures of the shafts of the bone in which reduction is not satisfactory, that operation will do our patient a world of good.

The use of blocks and tackle, as suggested by Martin of Philadelphia, may aid us in difficult cases to get efficient extension.

The special instruments as devised by Lane are very good, but the handles are so long, that, though made with the admirable object of keeping the hands of the surgeon away from the wound, they are difficult at first for those not used to them to handle.

I have found the iodine preparation of the skin most satisfactory. The skin is sterilized at night by the usual methods, and a dry antiseptic dressing applied. On the operating table the dressing is removed and the skin of the entire limb painted with tincture of iodine.

DISCUSSION OF PAPERS OF DR. COTTON AND DR. LUND.

Dr. D. F. Jones, Boston: It seems to be rather unfortunate that an enthusiast and a man of such wonderful technique as Dr. Lane apparently is, should be the advocate of the plating of fractures. In the first place his great enthusiasm in other fields has deterred many from taking up this work, and in the second place his wonderful technique has given him results which very few can hope to imitate. I have seen a number of men plate fractures within the last year, and I must say that not one of them came anywhere near the work Lane has done. Lane's work, as Dr. Lund says, is absolutely perfect. Now, before we can undertake to plate fractures we must approximate his technique, and it is very important that this work should be done by comparatively few men.

I agree with Dr. Cotton when he says, that because you have a fracture it is no reason it should be plated: I think he is perfectly right in saying that in many cases apparatus should be tried first. I think a great mistake is made in having only one x-ray taken before the apparatus is put on. If we would have an x-ray taken before the apparatus is put on, there must be one after, and another a little later, if necessary. Then if a satisfactory position cannot be maintained we should operate. In this way I think we could get somewhere near the cases that should be operated upon.

I am convinced that more and more fractures of the femur should be operated upon, and it seems to me there are also a good many fractures of the elbow which should be included. There are many fractures of the lower end of the humerus which can only be reduced and held in position by operation.

We have not time to go over all the fractures, but there are many which will be plated in years to come. But I agree with Dr. Lund, that if you have ever tried to plate one you will not think it so simple as it appears, and there will not be much danger of a furor of operating. It is at times a very difficult performance.

Dr. E. G. Brackett, Boston: The question of bone plating to-day seems to be one of the most interesting ques-

tions of bone surgery and probably is in a state of transition. The results, many of them, have been so brilliant that it cannot be condemned; yet, at the same time it seems too early to say anything definite about the permanent result of metal in bone. One would not think of opening up into a fresh fracture and plating it if the same result could be obtained by the conservative methods, but the question is, Under what conditions may we leave metal in the bone with safety? We know that it can be done under certain conditions, and some of these have been determined, and, as years go on, we shall know many more of them. It is already known that,—

(1) Plates must be used under the most rigid and absolutely aseptic precautions. They are for closed wounds only, and it is doubtful if they can be regarded as permanent tenants in wounds that have not healed by first intention and which have remained closed. Thus the careful closing of the wound is a fact of prime importance.

(2) The plate and the fragments must be absolutely firm. A loose plate becomes a foreign body and a source of irritation. This has probably been the most frequent cause of the removal of the plates.

(3) The plates must be large enough and strong enough to give a secure hold. Unless the fragments are held tightly in position the plate works loose and thus acts as a constant foreign body.

It must be remembered that bone plating is an outgrowth of the x-ray, and should be used only in conjunction with it. The x-ray must be used and several views taken in several planes to decide on the use of the plates, so that it may be an operation of precision, and in the majority of instances each step of the operation planned out beforehand. This is particularly true of all fractures involving joint deformities, which require correction of the distortion.

Dr. J. B. Blake, Boston: I should like to mention one point in regard to compound fractures, and that is, irrigating them with corrosive. In former years we used to open all the compound fractures and wash them with corrosive, and I believe that is a good rule. Then we swung from the antiseptic to the aseptic treatment of the wound, and we have all seen too many cases of sepsis follow in

compound fractures. I think that corrosive sublimate does good, and I should use it in all cases of compound fractures unless they communicated with the joint.

Dr. Cotton says that all fractures of the patella should be operated upon, and I think that with a single exception that is the best rule, in the great majority of cases.

It is a very strange thing that the operative treatment of fractures began with the operation on the patella which involves so large a joint surface. No one has any right to open a knee joint unless he is sure of asepsis. If you get sepsis in the knee joint the least you can look forward to is a wrecked joint, and the chance of death is very likely. If any one has seen sepsis follow a knee-joint operation he will never forget it, and he should not take the risk unless he is sure of asepsis.

In fractures of the patella Lane's plates should not be used, because the real condition is a tear of the patella tendon and in treating that tear the operator should follow almost entirely the treatment of Dr. Joseph Blake, of New York, who makes a point with Dr. Cotton, that not all fractures of the patella should be operated upon; that is to say, stellate fractures, where there is no tearing of the quadriceps, will do as well without operation. The tear should be closed with chromic catgut, or with kangaroo tendon for the aponeurosis over the sides of the patella, the most gentle manipulation of the joint should be carried out, and the pieces of the patella should be brought back into position. The refracture of a sutured patella is as frequent as of a patella that is not sutured. A figure of eight—or rather a half-circle suture on the sides of the patella will hold it in position, and I think if the point is borne in mind that you are doing a tendon and aponeurosis suture instead of a fracture the results will be better.

Passive motion may be begun under the surgeon's hand at the end of two or three weeks. In one of the cases at the City Hospital, at the end of two months the man was doing his own work, going up and down stairs, and the range of motion in the knee joint was only 20 per cent. less than normal.

Dr. Cotton: I am afraid it is my duty to explain that I am not opposing operative treatment of fractures where

there are definite indications. These cases that Dr. Lund has shown — I have seen most of them with him — there is no question about an operation in all of them, but there certainly is a question in some other cases. In many cases of fractures of the humerus and the femur there is no question about advisability of operation. In other words, we are not so far apart as we seem. Indiscriminate operation of all fresh fractures is what I am talking against. I am plating some of the old fractures, and I expect to increase the number of fractures I am operating on as time goes on, only I think we want to realize that there are risks involved; we do not want to be swept off our feet by the furor of operating. The furor has gotten into some medical circles and some of the hospitals; it is already here.

Dr. Lund is quite correct in saying that the operating on fractures is a difficult thing.

The man who has done the most work on this thing is Lambotte, of Antwerp. He is the man who first did much fracture operating; he used plates before Lane, I think, and worked out the problem, as we have seen it done to-day. So I want to set myself right in the matter of operation. I appreciate the advantages of the work, I am a firm believer in it and expect to do more rather than less, but I feel that we want to know why we do it. We want also to carry out the treatment by apparatus as it is not carried out to-day in any of the hospitals here, or in private practice. There is just as much for most of us to learn about fractures without operative treatment as with operative treatment.

Dr. Lund: I think that Dr. Cotton and I pretty nearly agree. I believe that certain fresh fractures of the femur will be operated on, perhaps without waiting for other treatment. In various parts of this country operators have been plating fractures for some years, but to Mr. Lane is due the credit of going about and doing the shouting for the thing. Enthusiasm does good in calling attention to good things; also, he has perfected the instruments for doing this operation and has put them on the market.

I agree with what Dr. Brackett has said about sloppy work. Now I know that some of my plates were not done as they should have been; I know the screws did not fit properly and the plates pulled off; and the more careful you

are about everything fitting the better results you will get; these things want to be absolutely firmly fastened. I happen to know that Dr. Brackett has used plates in a great many cases. He has used plates for fracture of the knee, has used pliable plates; and he said he very rarely had to take out a plate. Am I not right, Dr. Brackett?

DR. BRACKETT: Only one so far. I think the knee is an ideal place to plate, because you have absolutely square plates to deal with; and if you have a good place to deal with you will not have to take them out so often. But if you get good results, as compared with mal-formation, it is better to have used the plates even if they have to be taken out afterwards.

ARTICLE XIV.

CANCER OF THE COLON.

By HOMER GAGE, M.D.
OF WORCESTER.

READ JUNE 13, 1911.

THE cancer problem is still unsolved. In spite of the vigor with which it has been attacked in research laboratories all over the world, especially during the past few years, the additions to our actual knowledge of the nature, or the cause of cancer, are pitiably few. Many generous endowments have been established to encourage and facilitate its study, and special hospitals are being built, to be devoted to clinical observation and experiment.

All that has been accomplished so far, however, has been to attract attention to the interest, the importance, and the obscurity of the problem. Men and women have a peculiar dread of the very word cancer; and the terror which it inspires has been of late much enhanced by the newspaper articles relating to its rapid increase as a factor in our general mortality.

That this increase is real, and not imaginary, seems to be as certain as it is unfortunate. It is due in part, of course, to more accurate diagnosis: especially to the recognition of various forms of visceral cancer, which were formerly unrecognized;—and in part, to the increase which naturally comes with the growth of population. But our general mortality statistics, and the mortality statistics of the life insurance companies, plainly indicate, that it is a constantly and rapidly increasing factor in our death rate.

It seems quite clear, that the diminishing death rate from

contagious and infectious diseases, is to be matched by an increasing mortality in the middle periods of life from cancer, and the diseases of degeneration;—that of a 1000 infants, a much larger percentage may be expected to attain maturity, than would have beeen possible a generation ago;—but that of those who attain maturity, a smaller proportion may look forward to living out, what our older insurance tables have led us to believe, was their natural expectation of life. In this increased mortality of the middle and later ages, cancer seems certain to be an increasingly important factor.

No matter what may be our theory of its cause, I think we are pretty well agreed now, that cancer begins as a purely local process—that it is not a diathesis, and that it does not, at the outset, represent any constitutional taint. We believe, however, that from its original focus, it spreads, by invading all surrounding tissues, and in so far as its spread is thus confined, it is still a local process.

Very early in its history, the epithelial cells, which have become the cancer cells, are to be found in the efferent lymph vessels and glands;—a condition which is soon followed by a rapid dissemination through the lymph channels.

Now even if we could distinguish this process at the very outset, we have no means of preventing or of arresting it; but I think there is reason to believe that a thorough excision at this time, offers a reasonable prospect of a radical cure. There is certainly no remedy for cancer, except an early and thorough excision, and the prospect of securing a future immunity, is exactly in proportion to the promptness with which operation is undertaken, and the thoroughness with which it is carried out.

In the superficial and easily accessible places, like the skin, the lip, and even the breast, when a cancerous growth can be recognized early, and removed with a wide margin

of uninvaded tissue, our operative results are even now distinctly encouraging, and steadily improving.

But in the pelvis and abdomen, our efforts are still most unsatisfactory; not because our technique is bad, or our operative mortality excessive, but because in waiting for a clear and unmistakable diagnosis, we wait until secondary deposits, extensive lymphatic involvement, or the invasion of adjacent structures, prevent us from doing a sufficiently radical operation, and lead to early and progressive recurrence.

As a result, the internist and the general practitioner are very apt to feel that the danger, discomfort and disability attending surgical interference, are hardly worth the very brief respite which is all that can be reasonably expected. In many instances, such a position can no doubt be fully justified. In some, nature's limitations make improvement in results, so far as we can see now, well nigh impossible. But there are still some forms of visceral cancer that seem to present distinctly favorable opportunities for complete radical removal.

To one of these, I wish to call your attention very briefly;—not because there is very much that is new or novel to be said about it, or because my own experience with it has been large or particularly favorable, but simply because the more we study its anatomical relations, and the natural progress of malignant disease, as it appears in this location, the more promising it appears as a field for surgical interference.

I refer to cancer affecting the large intestine, between the ilio-cæcal valve and that portion of the sigmoid which lies above the pelvic brim. The most important recent contributions on the subject have been those of W. J. Mayo in this country, of Clogg in England, and of Petermann and Denk in Germany and Austria.

It is a sufficient commentary on the increasing importance

of the subject, to note that in 1887, Butlin could find but 32 cases of resection of the large intestine for malignant disease;—while in the communications above referred to, all of which have appeared within three years, Mayo presents the results of 69 consecutive resections,—Clogg, of 10,—Petermann, of 52, and Denk, of 39—each presenting only the cases occurring in his own clinic.

Such figures indicate very clearly, the earnest efforts that are being everywhere made, to study the living pathology of cancer of the large intestine, and to make its radical removal a safer and more satisfactory operation.

Reviewing the history of these later cases, we find confirmation of the old ideas, that age and sex are unimportant factors in determining the presence of malignant disease in this situation. There is a small preponderance of males; the proportion in Petermann's cases, being as 7 to 5,—in Mayo's, almost 2 to 1,—the average age would appear to be slightly over 50; the great majority of the cases occurring betweeen 40 and 70 —although Israel reports a case of cancer of the splenic flexure in a boy of 13, and Anschutz, two cases that were 16 and 17 years old respectively.

There is nothing in the histories to indicate that trauma, or even long continued irritation, are at all important predisposing factors, although I have within my own very limited experience, observed one case of carcinoma of the sigmoid, follow an ulcerative colitis of many years duration; and one case of cancer of the cecum occur, after many years of chronic intestinal distress.

The most striking fact brought out, by a study of the case histories, is the entire absence of any characteristic symptoms;—not only of such as might fairly be regarded as evidences of predisposing or contributing causes, but of any symptoms by which one might, with even reasonable confidence, recognize the early stages of a cancerous invasion.

In a strikingly large proportion of cases, the first indi-

cation of trouble, is an acute intestinal obstruction; thus of Denk's, 39 cases occurring in Von Eiselberg's clinic, 12 began with ileus. General constitutional symptoms, like loss of weight and strength, cachexia, etc., are present, if at all, only in the later stages. The presence of a tumor may be made, out in the region of the cecum or the sigmoid, if the abdominal walls are sufficiently thin;—colicky pains, with alternating constipation and diarrhœa, are rather more than suggestive.

But after all, the chief anatomical feature of these cases is the gradual narrowing of the lumen of the gut, and the obvious effect of such a narrowing is an increased constipation. Such a condition, occurring in an individual of suitable age, whose bowels have been in the habit of moving freely and naturally, ought, in itself, to arouse our suspicions.

If, in addition, it is associated with colicky pain, frequent, small and unsatisfactory stools, and especially with occasional attacks of diarrhœa, the possibility of a milignant stricture should be seriously considered, and the importance of an early exploratory incision recognized.

It is unfortunate that we have no more satisfactory means of arriving at an early diagnosis; but to wait for the appearance of an unmistakable tumor, or until the obstruction is complete, is to wait until the growth is too extensive to permit of its radical removal, or until the complicating ileus, converts the operation into an ante-mortem examination.

The encouragement to operate, and to operate as soon as the presence of the disease seems reasonably clear, without waiting for any stronger proof of its existence, comes from a study of its anatomical relations. It has become a cardinal principle of the surgery of malignant disease, that not only must the growth itself, but also the lymphatics, which drain the area in which it is found, be radically removed.

Failure to do this, is justly regarded as one of the prime factors, contributing to the early recurrence in many of our operations on cancer of the viscera.

In cancer of the large intestine, we are now in a position to follow the lymphatics, which drain the different portions of the gut, into the depths of the mesentery, and to remove them in favorable cases, well beyond the limits of the cancerous invasion.

The cecum and ascending colon receive their blood supply from the ileo-colic, and the right colic arteries, and both are accompanied in their course, by a chain of lymphatics, which are apt to be involved in cancer of this region. Their removal is likely to require ligature of both arteries, at their junction with the superior mesenteric, and cancer of the cecal region, therefore, involves the removal of four or five inches of the ileum, as well as of the ascending colon, nearly to the hepatic flexure.

Clogg has found glands also behind the cæcum, and in close contact with the ascending colon, which must be carefully looked for. The hepatic and splenic flexures, and the transverse colon, which lies between them, are supplied by the middle colic-artery. The lymph drainage of this area is not so directly in the course of the blood supply, but in much closer communication with the lymphatics about the pancreas, and along the side of the aorta.

Thorough removal of cancer of this region is, therefore, much more difficult than when the disease is in the cecum, and involves the ligature of the middle colic artery and the excision of as much of the colon as is supplied by it. Coming now to the descending colon, and that portion of the sigmoid which lies above the pelvic brim, we find their blood supply derived entirely from the inferior mesenteric artery, with the lymph drainage following the course of the vessels.

Moynihan's observation of a terminal gland at the root of

the inferior mesenteric artery, would make it logically necessary to ligature that vessel near its origin, and to remove all of the gut which it supplies; viz., the greater part of the descending colon, as well as the upper part of the sigmoid.

When, for any reason, such an extensive resection should seem inadvisable, and no enlarged glands can be felt beyond the immediate vicinity of the growth, a less radical operation undoubtedly offers a reasonable prospect of immunity from recurrence, and should be unhesitatingly tried.

It has been frequently observed, that cancer of the sigmoid seems to retain its local character much longer than cancer in other parts of the large intestine, and that lymphatic involvement and visceral metastases occur, much later. In 41 autopsies observed by Clogg, in less than 15% was there any metastasis, and that always in the liver.

A less radical operation may, therefore, be more properly undertaken in this locality, when, for any reason, the more radical one seems counter-indicated. The main thing is, that with proper regard for its blood supply and lymph drainage, cancer of the colon may be removed as thoroughly and as completely as cancer of the breast.

This could not be accomplished, however, if the colon could not be made easily accessible, but it can be made accessible, and this mobilization of the colon is the most important factor in the technique of the operation. All of the blood supply of the colon, as well as its lymphatic drainage, lies in the inner half of its mesentery; and it is possible, therefore, to divide the reflexion of the peritoneum to the abdominal wall, which constitutes its outer half, without interfering at all with its nutrition.

Its division makes it possible to strip the colon from its bed, and with its long inner half as a hinge, to bring it within easy reach of the operative wound. In stripping the hepatic flexure, especial care is necessary, to prevent injury

to the duodenum, with which it lies in close contact; and in mobilizing the ascending and descending portions of the colon, and the sigmoid, one has to be careful to avoid doing injury to the ureters.

With these dangers in mind, one finds it possible to see clearly the blood supply, to resect freely in full view, and at the same time to restore the continuity of the intestinal canal, by an end to end or side to side anastomosis. What kind of an anastomosis should be made, and in how many stages the operation should be done, are relatively of much less importance; the colon's tolerance of manipulation, and its accessibility, by means of mobilization, are the essential factors in making the operation possible and practical.

Statistics as to the mortality are especially unsatisfactory in these cases, because one is never sure of the conditions under which the operations were performed. In the carefully selected cases from Mayo's clinic, the mortality was but 13% ; and from this, the percentage rises, until, in the cases reported from Von Eiselsberg's clinic, when apparently no selection was made, it reached nearly 50%.

Excluding the cases of acute ileus, with great distension, and grave constitutional symptoms, which are practically hopeless, and ought not, I think, to be operated upon, the average mortality should approach more nearly the figures of Mayo, than of Denk.

The distinctly hopeful and encouraging feature, is the number of cases in which a very long immunity from recurrence has been secured. In Petermann's cases, 11 were free from recurrence more than two years after operation,—seven, more than three years,—and three, more than seven years; in Denk's, seven were alive and well more than three years after the resection, and in Mayo's, 10; a pretty large proportion of those who survived the operation; rather better than the average results in excision of the breast for cancer.

In a case reported by me in 1901, of resection of the sigmoid for cancer, the patient lived nine years, and died a violent death, without any sign of recurrence; and a case of cancer of the cecum, reported by me in 1903, was alive and well six years later, and has since been lost sight of.

It would seem fair to infer, from these figures, that cancer of the colon is, at least, no more malignant than cancer of the breast; that it has no greater tendency to lymphatic involvement or metastasis, and that an equally radical removal is possible.

To be sure, the operation requires a much higher degree of technical skill and wiser judgment, in the selection of appropriate cases; but when these are attainable, it would seem to offer a reasonable hope of relief, from an otherwise hopeless situation.

DISCUSSION.

Dr. J. G. Mumford, Boston: Dr. Gage's luminous paper cannot fail to interest all practitioners who are seriously concerned with this important problem. It is a familiar fact that statistics can be made to meet any desired contingency. The statistics quoted by Dr. Gage are sound and convincing, for they are the statistics of reliable observers. I cannot but believe, however, that the experiences of surgeons dealing with cases of cancer of the colon are so controlled by diverse conditions that uniform conclusions must be rare. Allow me to relate the outline of a case of cancer of the colon which followed a not unusual course. The patient was well known to me personally. Last March a distinguished physician of Cleveland, a man of the widest experience, keen observation, and sound judgment, in his forty-ninth year, was suddenly overwhelmed by intestinal obstruction. No recognizable premonitory symptoms had occurred; his attack was unforeseen. He consulted at once an able surgeon, who operated upon him and found an advanced malignant disease of the splenic flexure. Its removal was impossible. A few days later

the patient died. With such a case in mind, one cannot but reflect that early diagnosis and prompt treatment may be extremely difficult.

I cannot but believe that cases of cancer of the colon may in broad terms be divided into two classes: first, the well-to-do, observant private cases; and second, the poor, negligent, public hospital cases. I have made something of a study of the public hospital cases, and in order to ascertain the clinical facts as present in this community, I have reviewed carefully the records of these cases at the Massachusetts General Hospital, which doubtless correspond with similar public hospital records elsewhere. I find there recorded 48 cases operated upon during the past sixteen years. The rather ghastly record is:

1 case presumably well.
3 cases of which there is no end record.
44 cases dead.

From these reports it appears that the disease is short, and violent, and is a disease of young middle age.

All of these cases received expert care, and were divided among sixteen surgeons; the surgeon treating the largest number treated eight, which suggests the comparative rarity of the disease and the small experience in it of any one man.

As with most internal cancer the difficulty of early diagnosis is extreme. It appears that patients are usually far advanced with the disease before they themselves realize their illness. They consult the family physician, who discovers a tumor. Rarely does it happen that the disease when so advanced as to present a palpable tumor is capable of cure.

We hear various talk of the possibility of early diagnosis, and it is true that occasionally an early diagnosis may be made. What are the initial symptoms in the most favorable cases? They are two: pain and vomiting. Of our 48 cases, 37 complained of pain as an initial symptom, and the pain was usually located near the site of the disease.

Moynihan would have us believe that this pain is often characteristic, as though a hand were gripping the viscera and then releasing them. Patients are said to describe this characteristic pain. In our list of cases such descriptions

are rare, for the patients usually describe two types of pain: severe attacks of colic, recurring at intervals; or, second, a steady, dull, grinding, general abdominal pain.

Pain is due to peristalsis against opposition, rather than to the infiltrating of the growth involving nerves. Peristalsis endeavors to force the fecal masses past the stricture, and frequently succeeds in doing so, with the result that the patient passes marble-like stools.

The second cardinal symptom is vomiting; nausea is more common even than vomiting. Twenty-four of our forty-eight cases described recurring attacks of overwhelming vomiting: Vomiting is regular, recurrent, and corresponds largely with the attacks of pain. Fecal vomiting is rare.

In very advanced cases, there is the usual chronic nausea and loss of weight.

An extremely interesting and important feature of this disease is its appearance in early middle life. Twenty-two, or nearly fifty per cent. of our forty-eight cases, were under fifty years of age; six were in their thirties; two were in their twenties—the youngest being twenty-eight years old; eleven only were over sixty years of age.

The short duration of the illness, when it comes to the surgeon, is another very striking feature. Most of the patients measured their illness by months. Twenty-four, or fifty per cent. of the patients, had been ill less than six months; six had been ill less than eight weeks; and two had been ill but five days; thirteen only of the patients measured their illness by years—from one to two years. Many of these cases further are regarded by practitioners as acute inflammatory conditions. Indeed, the attacks suggest strongly acute gall bladder infections and diverticulitis. Two or three cases in my list had been operated upon for appendicitis before they entered the hospital. The youthful age of many of these patients also deceive practitioners. Few of us realize the frequency of cancer of the colon in young men and women.

The late symptoms, and the symptoms which usually confess the true diagnosis, are those of intestinal obstruction: pain, vomiting, obstipation, distention, often with rise of temperature. The extreme gravity of such a condition leads the physician at once to seek surgical advice.

Sometimes a tumor is felt before the operation. In twenty-one of our cases such a tumor was made out, and the diagnosis thus confirmed before the patient entered the hospital.

The site of these tumors is interesting; one-half of them, or twenty-four, were divided equally between the splenic and hepatic flexures.

Let the practictioner, therefore, search carefully the right and left upper quadrants of the abdomen, when he suspects cancer of the colon. The remaining 24 were divided variously, and in their order of frequency as follows: cecum 7, ascending colon 5, transverse colon 4, descending colon 4, sigmoid flexure 4. It is interesting to reflect that the hepatic and splenic flexures, the sites of most frequent colonic cancer, are the points most susceptible of damage by the advancing fecal stream, especially in cases of intestinal ptosis. Observe further, that if a tumor of the colon is noted before operation, such a tumor is a large one and its radical excision is rarely to be expected.

Of recent years, and in the hands of the most careful practitioners who see these patients, the finding of occult blood in the stools by the Guaiac test, has proved of the greatest value, as indicating the presence of malignant disease.

The operation. Shall we operate? and when shall we operate? It is a sad reflection that nearly 80% of our cases die within a few days after the operation, no matter what the character of the procedure. We get our patients late; their vitality is exhausted, and they are unable to stand the shock of abdominal section; mostly they die. Here again are a few figures: There were 13 excisions of the growth, 9 of the patients died; of the 3 we have no end report; 1 was well two years after the operation. That is the only case which can in any sense be regarded as cured, though the three cases not reported possibly may be well.

There were 10 cases treated by anastomosis; 6 died at once, and 4 were relieved of their symptoms but died subsequently.

Sixteen cases were treated by colostomy; 11 died at once—that is to say within the month—and 5 were relieved; 9 cases were explored merely, and died shortly after the operation.

These are melancholy statistics, but they are the average statistics, and are in no sense due to an ineffective technique. The patients died because they were exhausted by advanced disease.

One asks, then, would it not be best to let alone all patients with malignant disease of the colon, that they may die in peace. That is a question which admits of some debate. The one reason for letting them alone is that they may experience euthanasia, that they may die in comfort, drugged with opiates. As opposed to this method, we must reflect that many of these cases deserve an operation in order to determine the diagnosis. Furthermore, the bare statistics do not tell the whole story. Any man familiar with these cases, and the attitude of the patients after operation, knows that many times, although death impends, and although life can be prolonged but a short time, the comfort of some of the patients is increased and their mental outlook immensely improved. To obtain this favorable condition at least may be the object of the physician and of the surgeon. In other words, the surgeon must select his cases carefully, and must have wise surgical judgment regarding the probable outcome of operation.

In brief summary, I believe we may draw certain conclusions regarding this whole unfortunate group. We must, if possible, get the cases early and make our diagnosis promptly and accurately. We must excise whenever possible; I am convinced that excision of an early growth is not the necessarily grave undertaking which the excision of a late growth always proves. Some excisions of the colon or portions of the colon, when not diseased, have been shown to be relatively safe undertakings. I have myself removed the non-diseased colon in three cases of obstruction from ptosis, and I have learned that in such cases the shock is inconsiderable. Doubtless the same would be approximately true in the case of excisions for early cancer. Occasionally anastomosis is justifiable to relieve symptoms and to prolong life; and anastomosis or splicing of the large intestine, under any circumstances, should invariably be done by side to side stitching. Occasionally colostomy may be imperative to relieve suffering and prolong life; but even colostomy, as our figures show, carries with it a high mortality.

Dr. F. B. Lund, Boston: I think that we ought to look, as far as we can, on the hopeful side of these cases, for we all know the helplessness of a patient with cancer in any part of the body.

The matter of early diagnosis is very important, and the finding of blood in the stools ought to be carefully searched for by microscopic methods.

I have found cases in which there were acute attacks of intestinal obstruction, and these have not been the cases which have been immediately fatal in my hands. One case was obstructed for eight days, and all that was done was to make an incision and draw out the growth. The growth was resected during a second anesthesia, under cocaine, a clamp was put on it and the case went on for three years. Then there was a recurrence, and we cut out a portion of the sigmoid, and she went on three years more. She was the mother of a large family and it was important that she should live as long as possible.

I had another case, a young woman under 35 years of age; found an acute obstruction of the large intestine and the transverse colon dilated. It contracted and looked pretty well, with the transverse colon hanging down, and three weeks later I was able to resect the colon about half way down its length. I hope she is alive now; I have not heard of her death.

Another case, an obese lady about 60, was operated on two years ago, and I did not dare to take out the tumor, but made an anastomosis between the colon and the sigmoid. She lived in perfect comfort. In still another case of resection of the transverse portion of the colon, I resected the cecum and then the patient died of pneumonia.

These are cases I can remember offhand. The mobilization of the colon is spoken of by Dr. Gage. Tying the main vessels on the inner side is of great value in operating on the fixed portion of the colon. The favorable thing about cancer of the large intestine is that it grows slowly.

I do not think that cancer of the colon is very much worse than cancer elsewhere, although a great many exploratory laparotomies are done, and we find that the cases have gone so far we cannot do anything. In my opinion, the results in private practice will always be better than in large hospitals, because in the latter the patients are poor and they come to

these charity hospitals because they have not anywhere else to go, and they do not come to the hospitals until the disease is well advanced. I think that under these circumstances hospital statistics cannot be very satisfactory.

Dr. J. B. Blake, Boston: It seems to me a question not only of the ability of diagnosis with the general practitioner, but also of the fact that the patients do not speak of the slight symptoms that appear in the early stages. In case after case that I have seen, there was a considerable number of symptoms which, if we had known of them, would have aided in the diagnosis, but the patients did not mention them. Now if, in the smaller cities, the physicians who are like the old styled family practitioners could instruct their patients that any one who develops chronic indigestion, after 30 years of age, would submit to a thorough examination, I think we should make some progress in the early diagnosis. I think the general practitioner usually makes a diagnosis as early as his opportunity permits.

As to the value of palliative operations, that is very great if we include in the palliative operations the operations in which resection is done afterwards, and it seems to me that a much better recovery rate may be looked forward to than in the past.

A simple enterotomy has, in my cases, prolonged life for one or two years. A man may possibly carry on his work for one or two years, and his working ability is not interfered with by his operation. I have recently operated on a man who is a rowing coach and who is able to go on with his work. Certainly, in the cases in which it is done early, it has a definite result in prolonging life.

One other thing that Dr. George Gay has called to our attention, is the administration of iodin and bromide of arsenic. This diminishes pain and makes morphine in some cases unnecessary, and it has often been followed by a prolongation of life beyond what might be deemed possible at the time of administration. Iodin or bromide of arsenic may be taken for months, and in the inoperable cases I think the administration of these two remedies is essential and should be resorted to.

Dr. P. E. Truesdale, Fall River: I have operated upon ten cases of cancer of the colon, with two deaths.

Post mortem examinations in these two cases allowed an opportunity to determine the futility of the one-stage operation. A similar condition obtained in both patients. Fecal impaction was found involving the entire colon on the proximal side of the growth. Unless the operation of colectomy is done, preliminary colostomy for the purpose of thorough drainage of the colon offers the only reasonable hope of success in the radical operations for cancer of the lower bowel.

I recall a meeting at the Boston Medical Library during the winter when Dr. William Mayo read his paper on the treatment of ulcer at the pylorus and gave a very favorable report of his stomach surgery. It seems to me that the mortality as obtained in this same class of surgery in Boston is unwarrantably high, and leaves the average physician with a very pessimistic view of the whole subject. One who has observed the surgery at Rochester, Minn., and at Boston is impressed by the fact that the Mayos operate with a view to curing the disease, while in Boston stomach surgery has been emergency surgery, life-saving procedures, and divided among many operators.

In the paper read by Dr. Gage, the patient was operated upon in a period of election when an early diagnosis had been made and the necessary preparations carried out, whereas the cases reported by Dr. Mumford, surgical measures were in the main *dernier resorts*, and the immediate mortality was therefore very high.

Dr. Homer Gage: I have nothing to add particularly, except that I have been very glad to hear the remarks of Dr. Mumford. The difference between his point of view and mine seems to be that the cases that enter our great metropolitan hospitals are apt to be cases of cancer in its later stages, in many of which it would probably be better not to operate at all.

It seems to me perfectly evident that if we grant, what seems to be certain, that cancer of the large intestine is of slow growth, and that it is originally at least a local process, there must be a time in these cases when it can be successfully removed.

In addition, we know that the colon is very tolerant of manipulation, that it can be mobilized so as to be easily

accessible, a point upon which all the modern operators have laid great stress, and that it is not only possible to remove it entirely, but with reasonable prospect of long immunity if it can be done early enough.

ARTICLE XV.

SURGICAL TREATMENT OF PYLORIC STENOSIS, WITH REPORT OF CASES.

By S. W. GODDARD, M.D.
OF BROCKTON.

READ JUNE 13, 1911.

As a beginner in surgery, I have much hesitation in coming before so large and distinguished a gathering of physicians, to discuss so important a subject, which has received such ample and thorough treatment of late, in the hands of great men like Mayo and others. But it is with a feeling that at this time, and under these circumstances, it will not prove amiss to consider it again, especially by those of us who are in the outskirts of the large cities, that we may be able to diagnose and obtain early those favorable cases, which so far as we can now judge, give excellent results under surgical treatment; and also that we may show our teachers and those who are pioneers in this work that we are trying to get results by following their example.

I am greatly indebted to the late Dr. John C. Munro and to Dr. Bottomley for the unusual opportunities they afforded me, to study, assist, operate upon and observe stomach cases at the Carney Hospital, over a period of about three years. This experience, together with a chance to note their end results, has given me a keen interest in this line of work; and during the three years that I have been associated with my father at the Goddard Hospital in Brockton, I have operated on a few cases which have come under my observation there. These, in no way remarkable, and only

representing the ordinary run of cases of pyloric stenosis, caused by chronic ulcer of the stomach or duodenum, I wish to present for consideration. Cases of ulcer without stenosis, and those of stenosis without ulcer due to outside causes, I have omitted.

Case 1.

A woman of 42. For fifteen years has complained of indigestion and vomiting. Five years ago had an acute attack of epigastric pain, accompanied by vomiting of blood. In bed three weeks. Since that time her symptoms have been worse, almost constant distress coming on two to four hours after meals. Also much gas and sour eructations. For the past year has been unable to keep down any solid food, and has vomited much of her liquid diet. Very constipated, and has been losing weight rapidly. Has taken many kinds of medicine for a long period of time, and has never received any definite relief. She was a thin, pale, almost emaciated woman. On deep inspiration a tender mass was felt under the left costal border, otherwise the physical examination was negative. The stomach washings showed one pint of poorly digested residue, four hours after the ingestion of a full meal. Stomach capacity, 35 ounces.

She was operated upon in Oct., 1908. The pylorus was partly occluded by two hard nodules on the duodenal side, and would not admit the tip of the little finger. A few old adhesions surrounded it. At the cardiac end of the stomach, high under the diaphragm, was a firm hard mass two by three inches in diameter, firmly adherent, and inseparable from the liver. It was so extensive and its relations such, that it could not be excised. There had evidently been a perforation. A posterior gastro-enterostomy was performed by the Mayo no loop method with clamp and suture. She passed a normal convalescence, receiving full house diet at the end of the first week with no symptoms of distress or vomiting, and returned home on the 15th day. Seen one month after her discharge, she had no pain or distress after eating, but was troubled with gas after partaking of certain articles of food, such as potatoes or anything fried. Had gained five pounds, and was doing her housework, except the heavy work. Three weeks ago, could eat everything

without discomfort following. She never vomits, but is not wholly relieved of gas, which she belches occasionally.

Case 2.

A man of 32. For ten years has suffered from distress from food, coming on regularly two to four hours after meals. Belches large quantities of gas. Rarely vomits, never any blood. Stools normal. Feels best fifteen to twenty minutes after eating. Temporary relief obtained from vomiting, from pressure on the epigastrium, or on taking a hot drink. Drugs have afforded no relief, and often made him worse. He has had no acute attack, nor been laid up in bed. He is a muscular fellow, and the only physical sign noted was slight general tenderness over the epigastrium, more marked to the right. Stomach analysis showed no dilatation, and only three ounces of partially digested food.

Operation in May, 1909, revealed a slightly dilated stomach, and, on the posterior wall of the duodenum, three quarters of an inch from the pylorus, a definite thickening the size of a ten cent piece. This partially constricted the pyloric opening, which would hardly admit the finger tip. A few fresh and a few old adhesions ran from the pylorus to the omentum. A posterior gastro-enterostomy was performed. He passed a normal convalescence and was discharged on the 12th day with his previous stomach symptoms absent. Patient seen four days ago, and said, "Nobody knows what relief I have obtained, and how differently I feel." He has gained eight pounds, and is, and has been, free from all symptoms.

Case 3.

A man 46 years old. Has suffered from chronic indigestion for over ten years. Eight months ago had an acute attack of severe pain in the upper abdomen with vomiting, fever, and marked tenderness in the epigastrium. He was in bed, and kept on rectal feeding for three weeks. Then he gradually got better, but since has had regular distressed spells three to four hours after all meals and at night. Has been restricted to liquids for six months, and has habitually gone to bed with a hot water bottle on his stomach, and then suffered constantly. Has never vomited blood or

showed signs of it in the stools. Has taken many kinds of medicine, with little or no relief. He presented the appearance of a starved man. Had slight tenderness on pressure in the epigastrium, especially to the right.

At operation two years ago a dilated stomach was found. The pylorus was represented by a hard, firm mass the size of an English walnut, extending more on the duodenal side. It was evidently obstructed. There were many adhesions and much inflammatory tissue about it. No enlarged glands or evidence of malignancy. The stomach analysis showed marked stasis. Sixteen ounces of undigested residue were obtained four hours after a full meal. Capacity, fifty-six ounces.

A posterior gastro-enterostomy was performed as usual. His convalescence was interrupted by an ileus which lasted five days, with persistent vomiting and obstipation, probably caused by interference with the blood supply of the transverse colon, as he ran a normal temperature and pulse throughout. He returned home in seventeen days, eating full diet, and free from all symptoms. In two months he had gained ten pounds, and was still free from symptoms, except for an occasional distressed spell if he over ate. A small hernia developed in his wound and was repaired. Now he weighs 160 pounds, fifteen pounds more than at the time of operation, and is free from all symptoms except an excess of gas, occasioned by too free and rapid eating.

Case 4.

A woman of 48. Twenty years ago began to have indigestion and distressed spells coming on two to three hours after meals, accompanied by eructations of gas. Occasionally acute attacks of sharp epigastric pain accompanied by vomiting. Progressively growing worse, having lost twenty-seven pounds in the past eight months, and the symptoms increasing. In the past two weeks has been confined to her bed, and has vomited blood copiously, and passed large tarry stools. Thin emaciated patient, pale, hemoglobin, 55%. Slight tenderness to pressure in upper middle abdomen. No tumor or spasm.

Operation in June of last year. Marked stenosis from old thickened scar about pylorus. Bases of three ulcers felt, one on stomach side and two on duodenal. Stomach wall

thick, and considerable dilatation. A posterior gastro-enterostomy was performed and an uneventful convalescence followed. Freedom from all symptoms with full diet was experienced at her discharge on the 13th day. She was seen last week and had gained forty-one pounds since her operation. She eats, and has eaten everything and anything at any time, and never has had any distress. She says she is perfectly well and has done all her housework since the first month after her discharge from the hospital.

CASE 5.

A man of 44, who says that he has had stomach trouble all his life. When a boy he had sour eructations and heartburn, for which he got some relief from bicarbonate of soda. Five years ago he began to grow worse, and made the usual rounds of doctors. Pain and distress three to four hours after eating, bloating and eructations of gas, were his chief symptoms. He rarely vomited, and never vomited any blood. Some relief immediately after eating, never with any drugs. He presented the appearance of a starved man. Physical examination absolutely negative. A posterior gastro-enterostomy was performed October last, and a considerable degree of stenosis was evidenced by a thickened mass occupying the whole pylorus, about the size of a walnut. The stomach was enlarged, and its walls hypertrophied. He made a rapid and uneventful recovery, and was discharged on the 11th day, feeling no ill effects from full house diet. At present he eats every thing except potatoes and beans, which cause gas formation. He occasionally has heartburn when he lies on his right side, but it goes away immediately when he changes his position. Has gained 15 lbs., and says he has been made a new man.

CASE 6.

A woman of 40. Doctored for stomach trouble for the past twenty-one years, has been growing worse ever since, and is now losing weight and strength. She has received no help from any medicine except patent medicines. For the past six months she has vomited quite regularly, being unable to keep any food down without constant distress. At first it gives temporary relief to a gnawing feeling, but later causes sour stomach, heartburn and eructations of gas.

There have been two acute attacks, confining her to her bed for two to three weeks, but she has never vomited blood, or showed any sign of it in the stools. Slight general tenderness throughout the epigastrium is the only positive physical sign.

She was operated upon in March of this year, and an enlarged thickened stomach found. At the pylorus on the stomach side, posteriorly, was an ulcer size of a ten-cent piece, and anteriorly two small indurated nodules partially blocking the lumen. Gastro-enterostomy was resorted to. Convalescence was normal, and full house diet was taken before leaving the hospital on the 13th day. Now she eats three square meals a day without distress, sour stomach or vomiting. Occasionally there is a little gas. Meat and acids, which used to be her worst enemies, can now be taken with comfort. Gained 5 lbs. in the past two months.

Case 7.

Woman, 44 years old, who has complained of distressed spells and stomachache, coming on after meals for the past six years. She has obtained no relief from drugs, and is gradually growing worse and losing weight. No blood vomited, and no tarry stools. She says her food does not digest, and her worst discomfort is from three to four hours after meals. Some dysmenorrhea, with pain in the left side, has troubled her off and on. Her abdomen showed nothing abnormal on examination. A mass was felt to the left of the uterus on vaginal examination. At operation, March 11th, an enlarged, pale stomach was found. The pylorus was a mass of old, healed ulcers, especially on the duodenal side, with much constriction of the pyloric opening. Old scars and adhesions obliterated Mayo's sign. A posterior gastro-enterostomy was performed. An enlarged adherent pus tube was found, and not removed. She passed a normal convalescence, and returned home in 12 days free from her old stomach symptoms. At present she has no distress from any food she has eaten. Has gained 5 lbs. in weight.

Case 8.

A woman of 32, who came to me for operation for pus tubes, but who had been treated for six years for gastritis, during which time she suffered much distress in the epigas-

trium following her meals, and accompanied by belching of gas and vomiting. She would often be awakened at night, at a certain regular time, by sour eructations. No history of vomiting of blood. Frequent acute attacks of epigastric pain requiring morphine. These symptoms had been gradually growing worse, until she was restricted to a selected liquid diet, and had lost 18 lbs. in the past year. The tubal symptoms began three months ago. There was general tenderness in the upper and right abdomen, also over the pubes. Tender masses were found on vaginal examination.

At operation in March of this year, the stomach walls were found thickened. At, and on the duodenal side of the pylorus, on the posterior wall, was a thick nodule blocking the lumen so that a finger tip could not be admitted. A gastro-enterostomy was performed. Two large adherent pus tubes and the appendix were also removed. A normal convalescence followed, and she returned home in 22 days.

At present she is free from all symptoms of which she originally complained, and suffers no distress from regular full diet.

These few cases, very briefly and imperfectly cited, present pretty much the same type of symptoms, and uniformity in results. All patients were operated upon in middle age, and had suffered at least five years, and some for ten and twenty, before a diagnosis had been made. All, without exception, showed three or more of the leading symptoms of stenosis—distress coming two to four hours after meals, eructation of gas, loss of weight and strength. Vomiting has been unreliable, being late or absent, and hematemesis has been noted in only two cases, and tarry stools in but one. Constipation has been the general rule. The physical examination was only confirmatory and of little aid in diagnosis. Medical treatment at best has been only temporary, and in the long run a failure. The operation of choice in these cases has universally been gastro-enterostomy. Finney's operation would better suit a spasmodic contraction, or applied in cases where a gross lesion was

little in evidence at the field of operation, though in the hands of some, who are more skilled in his complicated techniqe, might readily have been resorted to in some of the cases. A gastrectomy did not seem warranted in any case. Had the ulcers been accessible, and had their removal not added materially to the gravity of the operation, they might have been excised. The no-loop operation, with clamp and suture, has been performed in all cases according to Mayo's technique, leaving an opening between the lower posterior wall of the stomach and first part of the ileum, that would readily admit two fingers. The abdominal wounds have been made around the right rectus muscle in the last five cases to avoid possible hernia, and to gain freer access to the right abdomen.

The operation, in no case, has been attended by so-called surgical shock. The anastomosis was done wholly outside the abdominal cavity, and the patient allowed to come out of ether, even to the point of semi-consciousness, and then re-anesthetized only to close the abdominal wound. The ulcer, or its remains, was in the majority of cases situated on the duodenal side of the pylorus; but in all cases, on, or within an inch of it. My patients, as a rule, have been allowed water, and albumen water, as often, and as freely as they desired, after coming out of ether. Rectal feedings have been abolished, and salt solutions given for a day or two, until mouth feeding could take its place. Semi-solid diet has been started on the third or fourth day, and house diet on the fifth or sixth. They usually get out of bed from the eighth to the tenth day, and go home in about two weeks, requiring no further administration of drugs. The convalescence was normal and uneventful in all cases, except that mentioned in Case 3. Hemorrhage or infection at the site of the anastamosis, has been absent in every case, and all wounds have healed kindly, including Case 3. All cases have gained weight and strength and almost total re-

lief from their previous symptoms. Each, with one exception, has reported to me in person during the past three weeks, the other indirectly through a friend, and all are personally responsible for the foregoing statements regarding themselves, and have expressed delight and gratification for their present condition.

While it would be absurd to tabulate data, or draw conclusions from such a meager group of cases, especially in this infant stage of stomach surgery, still, as a group, they coincide in symptomatology, treatment and results with those of experienced men; and, if they are a fair sample, I would ask, why we cannot get hold of more of these cases, and why we cannot get hold of them earlier. Eight typical cases, coming to one man, in one average sized city in this state, in two and a half years (and, I may add, that only one was sent to me), shows pretty conclusively that this condition is more common, and the cases more numerous, than we suppose. Ten and twenty years is altogether too long for a patient to suffer, if our hypothesis is correct.

Why should we wait for a tumor to develope, or muscular spasm and marked tenderness to appear, when in most cases the physical examination is indefinite and even negative? Why do we need to wait for the stomach tube to show a dilated stomach, or marked signs of stasis, when many of our patients suffer for years, before these can be demonstrated, though I do not underestimate the value of the stomach tube. Why should we wait for the presence of blood in the vomitus or stool, when in most cases we are unable to find it, at least at the time when the patient comes to us? If present, it is all important, but when absent, should not militate against a diagnosis. The absence of jaundice does not disprove the presence of gall stones, or the absence of hematuria, renal calculus. We have all been too much carried away by these remote, decisive symptoms, when the all-important part of the diagnosis, for which we

largely look to the general practitioner for aid, and the key note of the whole situation, is a careful, well-taken history in search of the early symptoms of ulcer, the most important of which is distress, coming on two to four hours after the ingestion of food (with comparative relief immediately following); accompanied by sour eructations, belching of gas, and gradual loss of strength and weight; and, the persistence of this symptom complex, month after month, not responding materially to the administration of drugs. When this is thoroughly and concientiously done, there will be fewer cases of indigestion, dyspepsia, gastritis, gastralgia, and the like, and it will give the surgeon a chance to effect, what now promises to be the best and only satisfactory method of treating pyloric stenosis.

DISCUSSION.

Dr. E. A. Codman, Boston: I think it must be a great regret to all of us that Dr. Munro could not have been here to have listened to Dr. Goddard's paper. I am sure there are a good many of you who recall Dr. Munro's first paper on gastric surgery, which was read at one of these meetings some years ago. At that time he advocated gastro-enterostomy in these cases, and was almost alone on the ground that he had taken. I am sure that it would have been a great pleasure to him to have listened to-day to the excellent case reports which his pupil, Dr. Goddard, has given, and I know he would have been very much pleased also at the splendid showing made by the Carney Hospital cases at the time Dr. Mayo read his paper here in the winter.

I am sorry to say my personal experience has been the reverse of Dr. Goddard's. Unlike his brilliant series of cases, I have had a very large mortality in mine. Much has been said by Moynihan and the Mayos about the advantage of dealing with living pathology instead of the pathology usually found on the autopsy table. My experience has been unique in offering me a chance to study my cases on

both the operating table and the autopsy table. I have lost nine cases of duodenal ulcer (including acute perforations). All of these have come to autopsy, so that I have been enabled not only to feel the induration from the peritoneal side of the operation, but later to see how the pathological anatomy of the lesion appeared when the lumen of the bowel was opened. The following points which I have obtained from this personal experience, and also from a careful analysis of all the cases which have come to autopsy at the Massachusetts General Hospital, seem to me worth emphasizing.

1st. That an ulcer which at operation appears to be very large, may at autopsy prove to have only a tiny crater, and to consist mainly of absorbable, inflammatory tissue, comparable to the indurated zone about a boil.

2d. Contrary to the experience of Moynihan and the Mayos, I find posterior ulcer more common than anterior. There may be puckering and a scar-like appearance of the anterior free surface of the duodenum, but the ulcer which is causing it all may be entirely on the posterior wall; I mean by posterior wall, the portion of the duodenum which does not have a free peritoneal surface. In my opinion, posterior ulcers defined in this way are more common than anterior.

3d. Another deceptive thing is the distance of the crater from the pyloric ring as determined by the pyloric vein. As a rule autopsy will show that the ulcer is much nearer the pyloric ring than it feels at operation.

4th. I believe that almost all painful ulcers are really sub-acute perforations. Such cases as Moynihan speaks of as being "tucked up," and those cases which have many adhesions about them or seem to be firmly attached to the pancreas, are really perforations, so that the base of the ulcer is formed by tissue which is not intestinal, but composed of the tissues of an adjacent organ or of the omentum. I was pleased to find that Dr. Mayo alluded to this fact, that sub-acute perforation occurs in a large percentage of his cases. I am inclined to think that an ulcer which has not perforated is relatively painless, so that much pain means sub-acute perforation.

5th. As to malignant changes. Moynihan and the Mayos say that cancer following ulcer of the duodenum is extreme-

ly rare. My belief is that this fact, though apparently true, is paradoxical, because cancer arising in a duodenal ulcer always spreads upward into the stomach. In other words, I believe that a cancer arising at or near the pylorus would tend to spread upward in the direction where the tissues were more or less devitalized by the condition of chronic stasis, rather than downward where there is no stasis and the onward movement of the food is not checked. Wilson and McCarty, in their discussion on the origin of cancer in ulcer of the stomach, say that the reason pathologists have not recognized the frequency of this condition before, is that the original ulcer is always covered by the time it reaches the autopsy table. In other words, they believe that the malignant disease tends to spread in the direction of least resistance, *i. e.* over the ulcer. The weakness of their position is that they do not apply this reasoning to ulcers of the duodenum which cause constriction at the pylorus, and may be the lower edge of the specimens which we call cancer of the pylorus. I have two specimens which indicate that this process has occurred. If I am able to obtain more such specimens, I shall publish something on this subject.

With the help of Dr. George W. Morse, I have recently been over the literature of stomach surgery since January 1909. In trying to be as judicial as possible in comparing medical and surgical statistics in ulcer, we have found that in the treatment of the individual attack, the medical statistics are distinctly better than those of the surgical as far as immediate cure and mortality are concerned. On the other hand, the evidence is in favor of the surgical statistics as far as permanency of cure is concerned. It is also true that the mortality in cases fit for operation is very low, and practically the only mortality is in the cases of "bad surgical risks." It seems only logical to say, therefore, that it is not the surgeon's duty to operate on ulcer cases which are in poor condition from an individual attack; that it is better for these cases to take their chances under medical treatment; that when they have arrived at a good enough condition to be "good surgical risks," they should then be operated on, with the idea of making their medical cure permanent. Logically, by treating cases in this way, the lowest mortality would be obtained, and the greatest number of permanent cures.

Unfortunately, the patient who has gone through a tedious medical cure and at length arrives at some degree of comfort, hesitates very much to put himself in the hands of the surgeon. But I am convinced that as time goes on the history of appendicitis will be repeated in ulcer, and that we shall operate, as far as we can, in the interval.

Dr. J. A. Gage: In discussing pyloric obstruction it may be well to recall a few physiological facts. The secretion of the stomach pours into the duodenum to meet pancreatic and biliary secretions as well as those from the gut itself, and these are all under control of harmones. Now it follows from the close interdependence of these functions that whatever affects one of these organs very frequently affects the others. Thus we have from unknown causes fatal hemorrhages into the pancreas, fatal dilations of the stomach, fatal sequels to biliary operations without adequate drainage, and fatal terminations to duodenal obstruction where the results cannot be laid to nervous shock or bacterial invasion. We know today that gastric secretions may be toxic by retention; that the same is true of biliary secretion; that pancreatic secretion is much more strongly toxic than either, and that obstruction to the first thirty-five centimetres of the duodenum will produce physiological death even as destruction of suprarenal glands will produce the same result. Furthermore, the distant internal secretions of the body seem to exert a powerful influence on the digestive functions, and diseases of the stomach seem to produce changes not yet understood, in the left lobe of the liver.

Now the one condition antecedent to disturbance in this tract is obstruction. The character of the obstruction may be either a temporary functional arrest from spasm of circular muscle fibres, or a permanent mechanical blocking, and yet a fatal result occurs in both instances. Probably most cases of pyloric obstruction are due to a combination of both conditions, the mechanical irritation leading to increased spasm. The result of this obstruction is a perversion of vital secretions to the lasting and sometimes fatal injury of the individual.

In seeking the causes of pyloric stenosis we have a wide range, from the congenital spasm and hyperplasia of the in-

fant to the cancer of the aged. Pylorospasm alone may have many causes, some in the alimentary tract, as ulcers, gall stones, appendiceal irritation, intestinal obstruction, and ptosis; others remote in the brain, pelvic organs, or possibly in the derangement of internal secreting glands. The importance attaching to this condition lies in the fact that its perpetuation by a constant cause (as chronic appendicitis) may so affect the health of the individual as to call for surgical intervention. In acute gastric dilation we may find the exciting cause in an operation on remote parts outside the abdomen, and yet the result may be rapidly fatal. Mechanical obstruction due to peritoneal bands, inflammatory exudates, growths, cicatrices, kinks, and in one case to a gall stone blocking pylorus, means lasting serious damage to the whole economy, covering long periods of time, and presents to the surgeon a large field. We think of the effect of obstruction as an interference with the digestion and assimilation of food, but it is far more than that; it is the retention of a vital secretion that is kept from playing its part in the common reservoir of the duodenum to become a toxic agent for the undoing of the individual, and the essential principle underlying treatment is the drainage of this toxic secretion from the stomach, just as we remove a gall stone or a prostate to release toxic secretions.

You wash out the stomach and you "cure" some of these patients temporarily — in acute dilation you do so permanently. As has been so aptly said, the cure of acute dilation should be made before its onset by washing the stomach at the first sign of post operative vomiting. When you institute drainage from the stomach, whether by way of the pylorus or by the jejunum, the result will be good because retention is overcome. But one fact seems to me important, and that is that we should not wait till a history of 12 years' sickness or food remnants appear twelve hours after eating before operation. To say nothing of the prolonged suffering, the appearance of the patient often indicates the profound impairment of the economy — an impairment that is not fully recovered from. By refinement of diagnosis and education of doctors and patients it must be made possible to operate earlier for the relief of these patients.

Dr. S. A. Mahoney, Holyoke: As the writer re-

marked, the appearance of twelve non-malignant cases within two years in the practice of one man, in one of the smaller towns of the Commonwealth is, to a man also living in a city of about the same size, a remarkable thing, and particularly when he reports results so favorable in every case it is still more remarkable. I know in our locality, looking back over a period of much longer duration, the results have been more in accordance with Dr. Codman's results. I do not remember, in an experience of ten years or so, having run across more than two or three cases which remained non-malignant up to time of operation. The majority of them at the time they come into our hands have passed beyond the stage where a simple gastro-enterostomy can be done, with no mortality. But of course every man's experience differs, and with the education of the medical fraternity the cases are coming into our hands earlier, and probably the future results will be more in accordance with Dr. Goddard's experience.

Of course Dr. Goddard's experience of two years is too short to have any end results. Relative to the occurrence of cancer in these cases, when I was at Rochester, Minn., some years ago, the pathologist at St. Mary's Hospital showed us a series of cases of cancer, where the cancer was grafted on the base of the ulcer, removed at the time of operation.

DR. GODDARD: In reply to the remarks of the last speaker, I would say that I am perfectly aware that the sound of my paper is very optimistic, and brings with it the suggestion of exaggeration, of picking out good cases, and omitting the failures or fatal ones, but I can assure you that the cases that have been presented are the only ones of that type that have been treated, and that the records and statements of the patients themselves will bear me out fully in the results.

The last point mentioned by Dr. Codman, I think, is an important one and should not be overlooked, and so I ask for the opportunity to show my results in stomach surgery in general and in full. Gastro-enterostomy has been performed on two cases of ulcer without stenosis, with results similar to those noted, except that one was suffering from tuberculosis, and now his tubercular symptoms are worse.

One case of perforated gall bladder, causing stenosis of the duodenum, was temporarily relieved by gastro-enterostomy seven months ago, and two weeks ago was operated on for gall stones, and is now convalescing. One man of 48 was operated upon two years ago for ulcer, having vomited blood profusely and presented most of the symptoms of ulcer. No ulcer or gross lesion was found, and the abdomen was closed. It was supposed to be a case of capillary bleeding. The patient got better, is now able to work, but is not free from distress and gas. I have explored one neurasthenic with stomach symptoms which had persisted for years, found no gross lesion and so closed the abdomen. On the whole she is some better, one and one half years after the operation.

This is the extent of my stomach surgery, but these cases, some of which may be called failures, cannot be grouped under the head of pyloric stenosis. As has been remarked by Dr. Codman, the end results are as yet far off, but as was mentioned in the paper, as a class they appear good so far as we can now judge.

SYMPOSIUM ON RENAL TUBERCULOSIS.

ARTICLE XVI.

PATHOLOGY OF RENAL TUBERCULOSIS.

By HUGH CABOT, M.D.
OF BOSTON,
AND GEORCE G. SMITH, M.D.
OF BOSTON.

READ JUNE 13, 1911.

In the short time at our disposal it will not be possible to discuss exhaustively, or even completely, this interesting topic, and no apology is made for apparent omissions of what seem the less important phases of the subject.

At the outset let it be distinctly understood that tuberculosis of the urinary tract is never primary in the strict sense of the word, that there is always another focus of infection in the body, generally either in the bronchial or abdominal lymph nodes. Whenever, therefore, the word primary is used in this article it will refer to the genito-urinary tract alone and not to the body in general.

AVENUES OF INFECTION.

(1) *Ascending Infection.*—It is not many years since the belief was widely held that tuberculosis of the kidney frequently resulted by ascension of the process from the lower urinary tract. It was apparently believed that tubercle bacilli had highly developed aquatic possibilities and could travel against the urinary current, much as a salmon ascends a river. Like many other fallacies in regard to urinary infection it was an attempt to explain a fact on false

premises, the error being due to a misinterpretation of the bladder manifestations. These are really due to late descending infections, though frequently the first lesions to produce symptoms. Increasing study with the cystoscope and ureter catheter has served to abolish this delusion, and to-day Kapsammer, who is particularly liberal minded in this regard, puts the frequency of ascending infection at one in sixty-two (62). As a matter of fact the belief in true ascending infection is based either upon experimental evidence of doubtful value, or upon a single case reported by Albarran, which showed briefly the following conditions:— There was an abnormal development of the left side, the kidney was divided into two parts, each having a separate ureter, with separate entrance into the bladder. At autopsy, the right kidney, ureter and that portion of the bladder lying about the right orifice, were found tuberculous. The upper division of the left kidney, with its ureter and surrounding mucous membrane of the bladder, were also tuberculous. The lower segment of the left kidney with its ureter and surrounding bladder mucous membrane were entirely normal. Albarran argues that the infection began in the right kidney, involved the bladder and then ascended to the upper segment of the left kidney. It has, however, been pointed out by Brongersma, that it is quite as likely that the infection of the left kidney may have been simultaneous with the right, or may have followed that of the right through other channels, and he pertinently inquires, why if the infection of the left kidney was secondary and ascending, had the lower compartment of it been spared, especially as the ureter orifices on the left were near together.

We are inclined to agree with his view, that this is a somewhat slender support for the doctrine of ascending renal tuberculosis, and upon the evidence as at present submitted we are by no means willing to affirm its possibility.

(2) *Hematogenous Infection.*—With the waning belief

in ascending infection the hematogenous route became generally accepted as the common avenue of entrance, and it is to-day supported by the most important special workers in this field, among whom may be mentioned Albarran, Legeau and Kapsammer. Important experimental evidence published by Albarran, Hansen and others supports this view, and it is certainly in accord with our belief in regard to joint infections in tuberculosis, notably the hip joint. That it is, however, open to more or less serious objections has been recently pointed out by Brongersma, who has, to say the least, cast serious doubts upon the accepted doctrine. He points out that if the bacilli were borne by the blood stream the lesions should be bilateral in a large proportion of cases, whereas the occurrence of unilateral involvement is the rule, not the exception. He has further shown that were the infection blood borne it should affect chiefly the cortex, and to be more or less evenly distributed throughout the organ, as in miliary tuberculosis, whereas the majority of tubercular lesions of the kidney are unilateral and occur in the medullary portion. He goes on to show, that in experimentally produced lesions trauma or stasis have been necessary, and that these factors are notably lacking in the patients as ordinarily seen. Trauma, kidney mobility and passive congestion are factors only in a very small percentage of clinical cases. After some study, we are inclined to accept the force of his arguments and admit that they shake considerably our belief in hematogenous infection as the common method. It must, however, for the present at least, remain as a route having the greatest number of adherents among those entitled to express an opinion.

(3) *Lymphogenous Infections.*—The lymphatic route as a method of infection is the newest comer in the field and is earnestly advocated by Brongersma following the earlier work of Tendeloo. It is admitted as an occasional

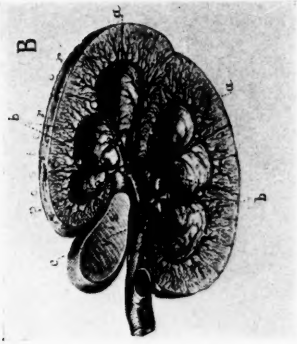

FIGURE I.

PICTURE FROM BROEDEL, illustrating the circulation of the kidney.

Note especially the formation of loops by the veins at the junction of the medullary and cortical portion.

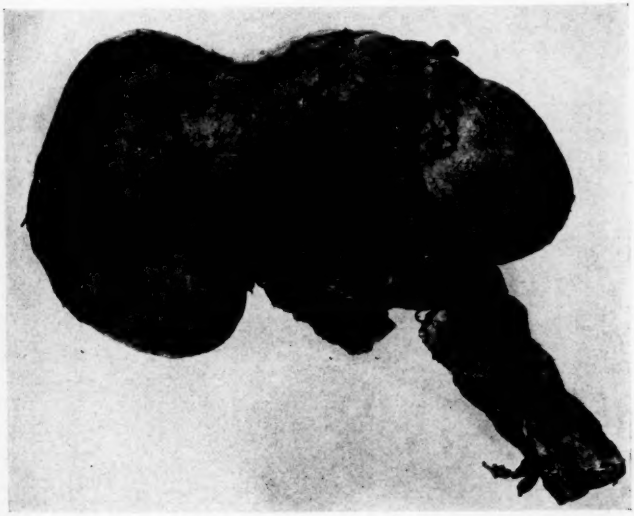

FIGURE II.

CAVERNOUS TYPE OF RENAL TUBERCULOSIS.

External view of the kidney and upper portion of the ureter showing gross appearances.

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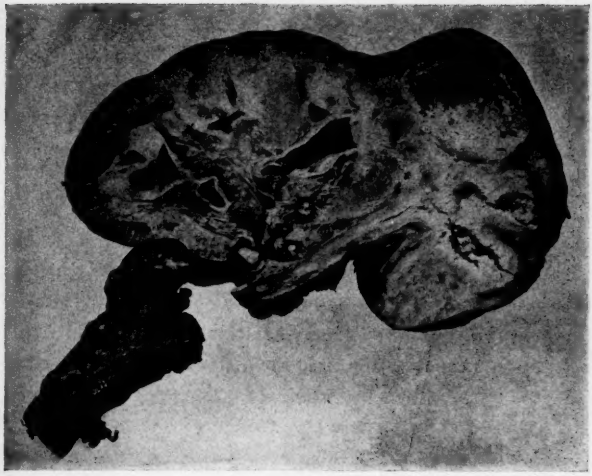

FIGURE III.

CAVERNOUS TYPE OF RENAL TUBERCULOSIS.

Cross section of the same kidney shown in Fig. II.

Note that the cavity formation has taken place in the middle portion of the kidney at the junction of the cortical and medullary portions.

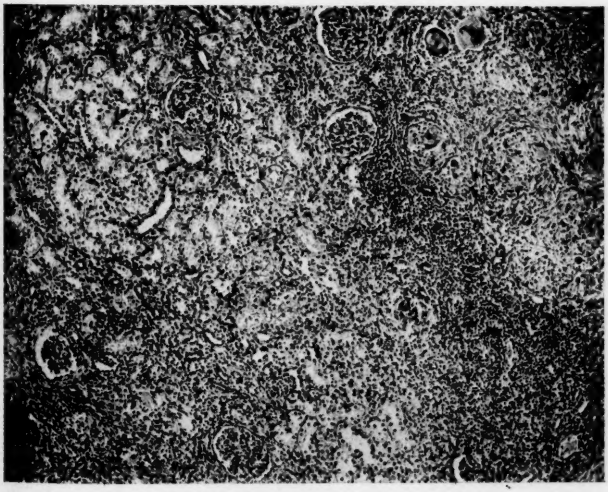

FIGURE IV.

RENAL TUBERCULOSIS.

Photomicrograph of a section of the lower pole of kidney shown in Fig. II. There are here to be seen typical tubercles with giant cells. The central portion of specimen shows fairly normal kidney tissue, while below is an extensive round cell infiltration.

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method by Albarran, but was strenuously opposed by him as the common method at the last meeting of L'Associatione Internationale d'Urologie.

The theory of lymphatic transference of the infection is based upon evidence furnished by two autopsies, in which it was shown that pigment entering the lung passed to the bronchial and mediastinal lymph nodes and thence to the para-aortic nodes. This route is apparently "against the current," but reversal of the lymph stream has been shown to occur in the presence of pleural adhesions, particularly of the diaphragm. Out of the seventy-one (71) cases of unilateral renal tuberculosis, pleural adhesions and lung involvement were found upon the same side in sixty-two (62). It has been pointed out that the lymph channels in the kidney follow the course of the renal vein and form an arch between the medulla and the cortex, which receives ducts from both portions of the kidney. In those cases in which infection of the kidney follows previous tuberculosis of the prostate or epididymis, the lymphatic channels may be important factors in the transference of the bacilli. From the prostate the bacilli may reach the perivesical lymphatic vessels accompanying the inferior vesical artery, and thence readily reach the para-aortic lymph nodes. Lymphatic infection from the epididymis is even more likely to occur as the bacilli may follow the direct course of the lymphatics accompanying the spermatic artery, which brings them into the immediate neighborhood of the kidney. The opponents of the theory of Brongersma point out that cortical lesions in renal tuberculosis occur not infrequently in unilateral cases and that they adopt the methods of their brethren of the legal profession, in entering general denial of the facts.

We confess that as a theory it seems to us interesting and important, as it accounts, as we shall later show, for the very common occurrence of tubercular lesions in the kidney at the point where the renal vessels form loops (Fig 1),

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between the medulla and the cortex, where the lymphatic vessels are most numerous and where lesions of lymphatic origin would be most likely to be found. This theory is strengthened by the absence of involvement in the neighborhood of the glomeruli, where hemotogenous infections with other bacteria are common and where toxic substances carried by the blood are most likely to affect the kidney.

FORMS OF RENAL TUBERCULOSIS.

(1) *General Miliary Type.*—This type we need not discuss here, as it is but part and parcel of general miliary tuberculosis involving both kidneys along with the other tissues of the body. It has no interest from a surgical point of view.

(2) *Cavernous Type.*—In this type the lesions occur along the line between the medulla and cortex. They frequently begin as isolated lesions which caseate and form either a cavity or a caseous nodule, generally in one pole of the kidney. It may break through into the renal pelvis and form a pyonephrosis, and frequently goes on to the formation of other caseous nodules which gradually involve the whole kidney and convert it into a cheesy mass. (See Figs. 2, 3, 4, 5, 6, 7 and 8.)

(3) *The Ulcerating Type.*—This process apparently begins in the papilla, soon breaks through into the renal pelvis, destroying the papilla and freguently resulting in tubercular pyelitis or pyonephrosis. It involves the pelvic portion of the kidney more extensively, more commonly results in pyonephronis, and is more likely to show active bleeding and cause symptoms. Should the ureter become obstructed dilatation of the pelvis and thinning of the kidney substance will occur. (See Figs. 9 and 10, 11 and 12.)

(4) There are an infinity of peculiar forms not exactly corresponding to any of the above types which are described

by various observers as constituting pathological entities. This is difficult to establish, and we do not regard them as sufficiently important to delay us here.

INVOLVEMENT OF THE OTHER URINARY STRUCTURES.

Sooner or later, in the vast majority of cases, the process involves the ureter and the bladder, though it may exist for years in the kidney alone. The pelvis of the kidney may be involved or a tubercular lesion in the medullary portion break through and infect the lower urinary tract. The ureter may become infected through its mucous membrane by the bacilli in the urinary stream. When this occurs the disease is most likely to take root at one or two points. (1) The physiological constriction just below the pelvis. (2) The bladder orifice of the ureter. This is perhaps because urinary stasis in some degree occurs at these points. Involvement of the ureter may also take place by spreading of the disease in the sub-mucous connective tissue following the pelvic lesions. (Fig. 13) This is a slow moving process and generally shows much periureteritis. It is probable that there are also cases in which the involvement of the ureter takes place by the lymphatics, and this perhaps explains the very marked periureteritis with thickening of the sub-mucous connective tissue, but without marked disease in the mucous membrane, which is so frequently seen in advanced chronic types of renal tuberculosis. The infection of the bladder most frequently begins about the orifice of the infected ureter and spreads from this point.

PROGRESS OF THE DISEASE AND ITS RESULTS IF UNTREATED.

We have thus far dealt only with the methods of infection and the lesions produced in the kidney, but it is proper that we should consider in closing the progress of the disease if not interfered with, with a view to pointing out where the surgeon should step in.

As with tuberculosis elsewhere the rate of progress varies enormously. It may go on rapidly to complete destruction of the kidney and infection of the other side. It may rapidly destroy the kidney without involvement of the other structures, or it may long remain confined to one kidney, or one portion of the kidney, making very little progress. We have now under observation a patient with undoubted renal tuberculosis of 11 years standing, where the amount of tissue destruction is still very moderate. A late result, and one of essential importance, is the effect of the disease upon the other originally sound kidney. This result may be of two kinds. First, the subsequent infection of the kidney with tuberculosis, and second, the so-called toxic nephritis, to which Albarran has particularly called attention. That the involvement of the opposite kidney in the disease takes place is well recognized, but it has only recently been widely known that this involvement is as a rule late. Many years may elapse between the time of infection of one kidney and the ultimate involvement of the other, and the tendency of the disease to long remain unilateral is perhaps its most important characteristic. The infection of the second kidney comes about practically in the same way that the first is involved. We are not inclined to give the ascending route much more prominence in this than in the former case, though the possibility of ascension by the lymphatics surrounding the base of the bladder and communicating with the para-aortic lymph nodes must be seriously considered.

SO-CALLED TOXIC NEPHRITIS.

This process affecting the other kidney is apparently the result either of the inability of the sound kidney to excreet an overwhelming amount of toxic substances, or as seems to us more probable, an evidence of its inability to do the whole work of the body under conditions abnormally try-

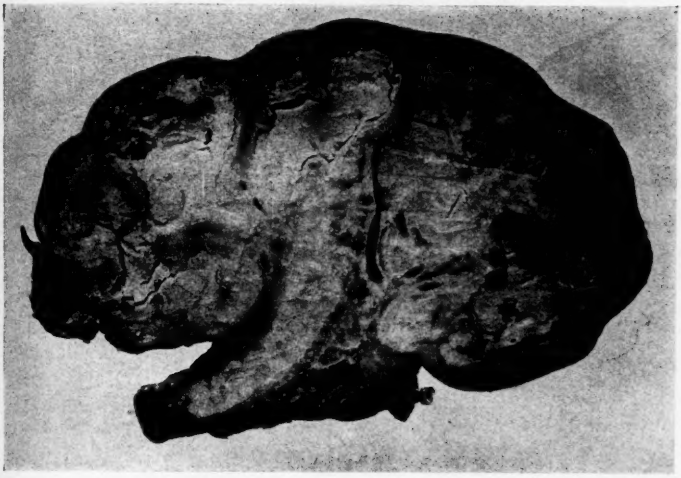

FIGURE V.
CAVERNOUS TYPE OF RENAL TUBERCULOSIS.
This specimen shows a more advanced stage of the process, similar to that in Fig. II.

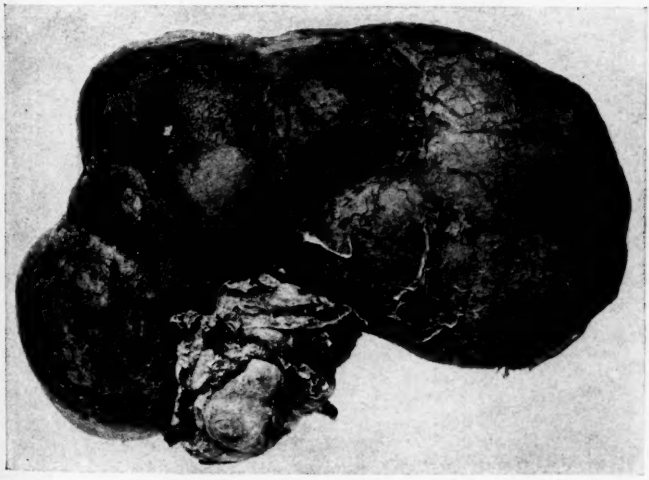

FIGURE VI.
CAVERNOUS TYPE OF RENAL TUBERCULOSIS.
External appearance of gross specimen.

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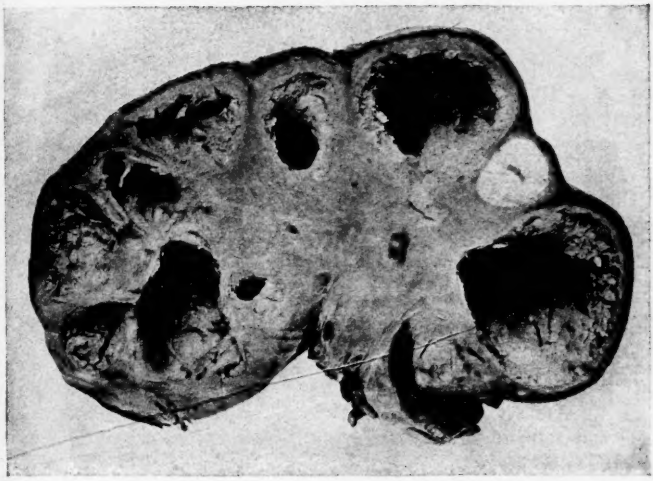

FIGURE VII.

CAVERNOUS TYPE OF RENAL TUBERCULOSIS.

Cross section of same kidney shown in Fig. VI.

Note the extensive cavity formation by softening of the central portion or caseous nodules.

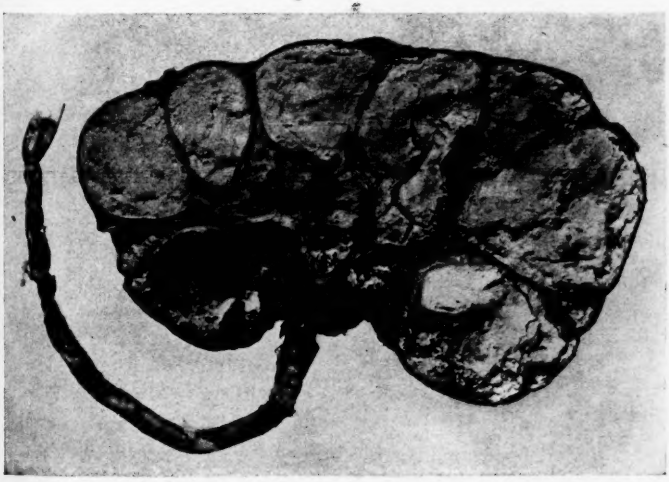

FIGURE VIII.

CAVERNOUS TYPE OF RENAL TUBERCULOSIS.

Cross section of kidney practically entirely converted into a caseous mass. Note especially the fibrous tissue partitions dividing the kidney into lobules. This was a practically dead kidney causing no symptoms.

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ing. Its pathological manifestations are similar to those produced by other toxins. It is essentially a glomerulo-nephritis with greater or less involvement of the parenchyma, producing clinical disturbances akin to those of chronic nephritis. With this is frequently combined a dilatation of the ureter without demonstrable obstruction, a phenomenon to which we would call attention, though unable at present to point out its method of production.

SPONTANEOUS CURE.

The most important question here to be considered is the possibility of spontaneous arrest or cure of the disease. We well know that the disease may go on to apparent clinical cure, by a process spoken of as spontaneous nephrectomy, by which the kidney is completely destroyed or converted into a caseous mass, which contracts and may leave nothing but a shriveled sac, in no sense a serious danger to the patient. This cannot, however, be regarded as a cure, except in the same sense that cure results from nephrectomy procured by art. The question of real importance is whether or not the disease, once having affected the kidney, may be arrested short of total destruction of the organ. There is much clinical evidence tending to support the view that cure without destruction may take place, but generally the evidence is incomplete. For instance, there are many reported cases of patients known to have had tubercle bacilli in the urine, symptoms referable to one kidney and evidence of suppuration in the urinary tract. These patients having been put under more favorable hygienic conditions, later arrive at a condition where no evidence can be found of the infection anywhere in the urinary tract. This can not, however, be taken as evidence of cure. In the first place, we are by no means certain that the kidney was involved, and in the second no evidence is supplied to prove that the kidney is still extant and sound. It has

been the custom, however, to point to these cases as evidence of cure, and we desire here to record our opinion that such evidence, though interesting, is totally insufficient to establish the fact, which in order to be of scientific value must include a positive demonstration of the approximate soundness of the same kidney with the absence of evidence of suppuration and of tubercle bacilli. This evidence can only be supplied by the accurate use of the ureter catheter and guinea pig inoculation. As far as we are aware no such evidenee has as yet been presented.

As further evidence of spontaneous cure, many pathologists have pointed to certain scars upon the surface of the kidney and stated their belief that these represented healed focci of tuberculosis. Within the last few years it has been frequently stated by pathologists of established reputation that renal tuberculosis may heal without destruction of the organ, but upon inquiry their evidence has been only that of small scars near the cortex of the kidney. Microscopic examination of these areas shows nothing but scar tissue, in no respect characteristic of renal tuberculosis. The position of the scars is not that ordinarily occupied by lesions of tuberculosis, and there is nothing about them to suggest that they may not have been produced by other agents. With the desire to prove that tuberculosis may heal we have the deepest sympathy; with the abundant clinical evidence tending to support the view we are entirely familiar, but we believe that at the present time no evidence has been submitted which establishes the fact beyond reasonable doubt. As far as we are aware no specimen has ever been presented before any scientific body which was generally admitted to show healed lesions of tuberculosis in the kidney. Until such a specimen is submitted we believe that the question is still a mooted one, and that the assertion that renal tuberculosis may undergo spontaneous cure is, as yet, unsupported by convincing evidence.

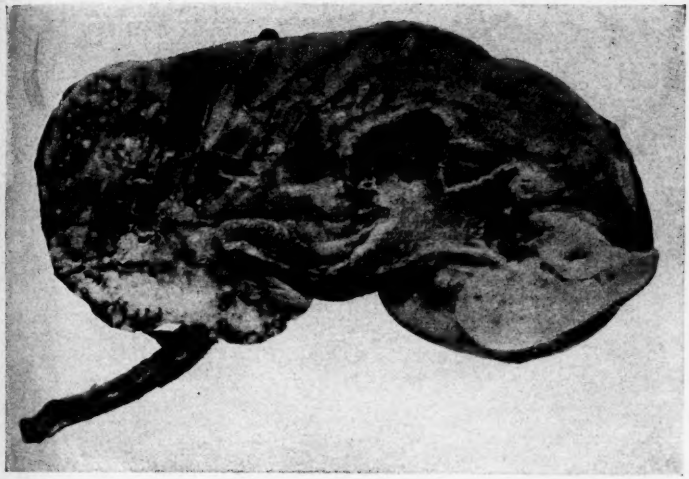

FIGURE IX.
ULCERATING TYPE OF RENAL TUBERCULOSIS.
Note the extensive involvement of the pelvis, the destruction of the papilli in the central portion and the scattered nodules of the disease in the lower pole.

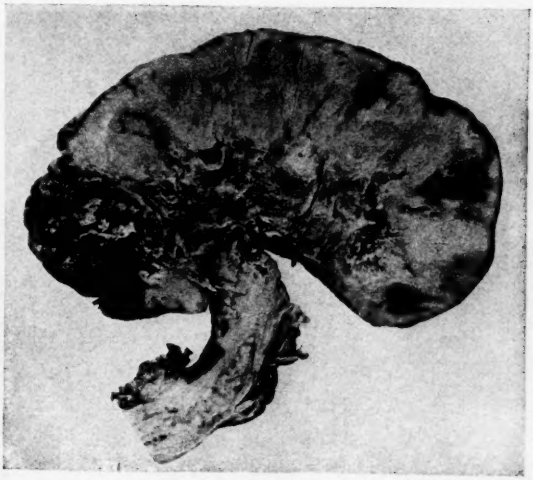

FIGURE X.
ULCERATING TYPE OF RENAL TUBERCULOSIS.
The disease here involves chiefly the papillæ and the pelvis, with a large necrotic area in the lower pole. There is extensive involvement of the ureter.

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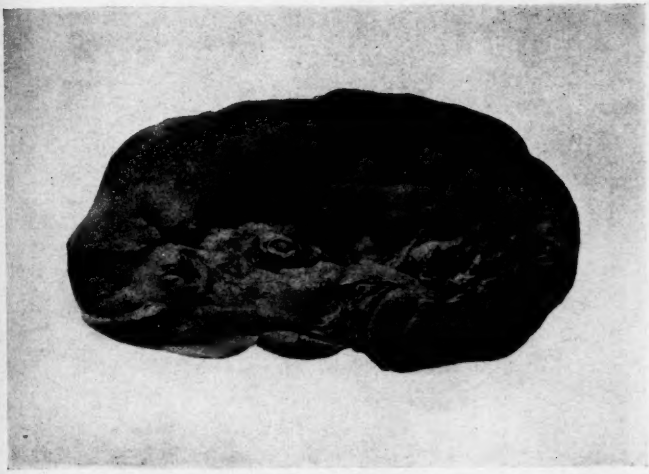

FIGURE XI.

ULCERATING TYPE OF RENAL TUBERCULOSIS.

This specimen shows an early stage in the disease. One papilla at the upper pole is considerably excavated and another at the lower pole shows the beginning of the same process.

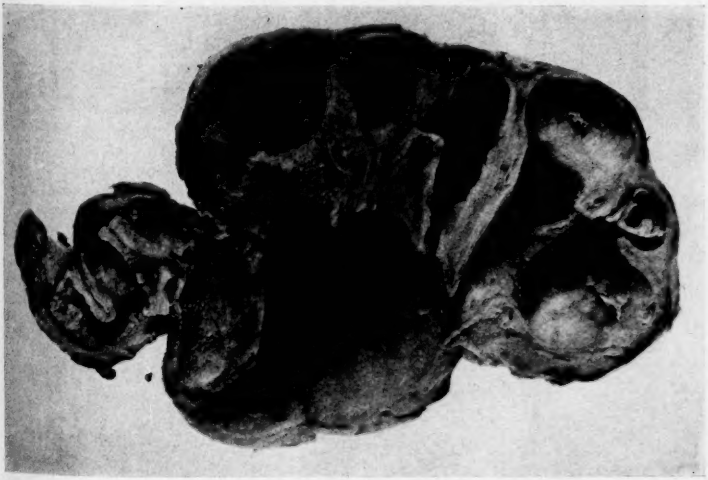

FIGURE XII.

PYONEPHROTIC TYPE OF RENAL TUBERCULOSIS.

This specimen shows obstruction of the ureter some distance below the pelvis with extensive thinning of the kidney substance and dilatation of the pelvis due to back pressure.

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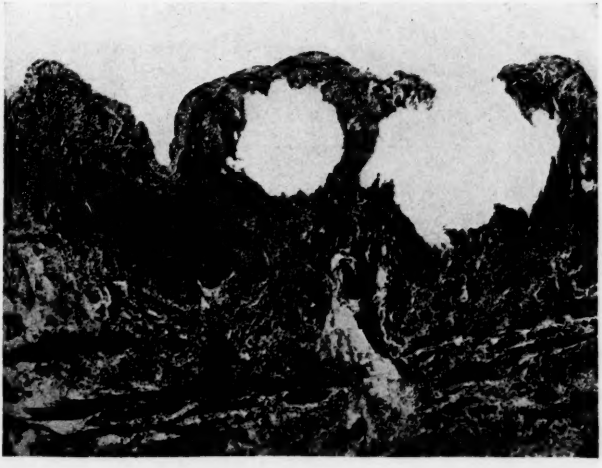

FIGURE XIII.

TUBERCULOSIS OF THE URETER.

Photomicrograph showing the formation of tubercles in the submucous layer, lifting the mucous membrane upon their surface.

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SYMPOSIUM ON RENAL TUBERCULOSIS.

ARTICLE XVII.

SYMPTOMS AND DIAGNOSIS OF RENAL TUBERCULOSIS.

By ARTHUR L. CHUTE, M.D.
OF BOSTON.

READ JUNE 13, 1911.

In my experience renal tuberculosis is the commonest cause for which we undertake major operations upon the kidneys, and nephrectomy, the operation usually demanded by this condition, is a serious procedure. It is probable that renal tuberculosis is at first unilateral in a large percentage of cases. I have found it unilateral in 70% of my personal cases, and Israel reports it as unilateral in 76% of his cases. On the other hand, in a series of 250 collected cases that came to autopsy, only 50% of unilateral cases were found. If we may draw any conclusion from this, it is that the longer the cases persist the more likely we are to find both kidneys involved.

The fact that renal tuberculosis is common, that its accepted treatment requires a serious surgical operation, that when overlooked it may pass beyond the point where satisfactory treatment is possible; all these things demand that we be able to recognize the condition, to recognize it with certainty and to recognize it early.

Kümmell, in a recent paper, has spoken of renal tuberculosis as being among the most difficult conditions to diagnose, and I believe any one who has seen a considerable number of these cases will agree that it is a condition in

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which an error of diagnosis is often made. The chief reason for this is that in by far the greatest number of cases the symptoms point to the bladder rather than to the kidney.

A year or more ago I analyzed the symptoms in those of my own cases of renal tuberculosis, in which my records were fairly complete. I have not added my cases seen since that time, but I do not believe they would materially change the figures. These figures illustrate, in what I believe is a striking manner, this predominance of bladder symptoms in renal tuberculosis. Thus, in 100% of my cases there was a turbid urine. In some this turbidity was slight, and in 10% was present only intermittently. In 100% there was a positive test for albumin by heat; in many instances the albumin was out of proportion to the quantity of pus. Pus was always present, and usually blood; the latter often only in microscopical amounts, but occasionally in sufficient quantity to term the urine "bloody." On the other hand, I found casts in none of these urines, though I do not for a moment doubt that they were present in some of these cases. Further, 80% of my cases showed both frequent and painful urination. In 15% there was frequent urination, but it was not attended with pain. In 5% of these cases there was no disturbance of micturition.

Pain that was considered renal was present in 45% of these cases, but in only 35% was this pain referred to the kidney that was found diseased. An enlarged kidney was found in 35% of these cases, but in 10% of them the enlargement was on the well side, that is, there was enlargement on the diseased side in only one fourth of the cases. Tenderness that seemed abnormal was present in only 20% of the cases, and in half of these it was referred to the well side. As the estimation of slight enlargement or tenderness depends largely on the examiner, errors may easily occur, and the personal equation must be borne in mind in accepting these figures. In the matter of pain, on the other

hand, I have accepted the patient's word as to whether it has been present or not.

Percentages are difficult to carry in one's mind, and they will doubtless vary a little in different series of cases; but the gist of this analysis is, that in less than one half of my cases of renal tuberculosis was there any symptom that pointed directly and unmistakably to disease of the kidney, though it was the underlying and important lesion. It is not surprising that errors occur in the diagnosis of a disease in which the symptoms often do not point to the organ involved, but do point strongly to another.

There is an added element of confusion in the fact that in a proportion of the cases that present symptoms pointing directly to the kidney, such as pain, tenderness or enlargement, this sign points to the well kidney and not to the diseased one. This misleading symptom is accounted for by the congestion due to compensatory hypertrophy of the well kidney. Only a short time ago I saw a case in which the diagnosis of inoperable double renal tuberculosis had been based on bilateral renal enlargement and tenderness in a patient whose urine showed many tubercle bacilli. When I came to catheterize this patient's ureters, I found that the urine from one kidney was normal and showed no signs of any inflammatory process, and that this kidney was doing about eight times the amount of excretion of the other side. The enlargement and tenderness were due to compensatory hypertrophy. This compensatory hypertrophy may lead us into two sorts of errors: First, as in the case referred to, of not giving the patient his best chance to get well by leaving the diseased kidney, and, second, by removing the kidney that is not diseased, under the impression that its enlargement and tenderness indicate disease. A kidney that is very painful, much enlarged or especially tender is almost without exception diseased; one that shows a moderate grade of pain, enlargement or tenderness may simply indicate compensatory hypertrophy.

It is evident, I think, from the foregoing that, if we are to recognize renal tuberculosis, and recognize it early, which is most important for successful treatment, we cannot depend on symptoms that point directly to the kidney, since they appear in less than one-half of the cases.

The symptoms that we do see, almost without exception, are a turbid urine, together with frequent and usually painful urination. These are the symptoms of cystitis, a condition that is common and due to many causes. There are certain things that should lead us to suspect that the signs of cystitis in a given case may in reality depend upon a renal tuberculosis. Thus a cystitis that appears without a definite cause is suspicious. Especially is this so in men. Women are more or less subject to "causeless" infections that ascend the urethra, but in men these are very rare. Any thickening of the cords, epididymes or vesicles heightens the probability. A very painful cystitis, or one that does not improve under appropriate treatment, is suggestive. A cystitis that has been present over a series of years, or a chronic cystitis that is attended with a temperature, should suggest the possibility of renal tuberculosis. On the other hand, we may not reason that, because there is no appreciable rise of temperature, the process is not a renal tuberculosis. A temperature was noted in 30% of my cases. Because there was a definite cause for a cystitis in the beginning, we may not assume that a long standing case is non-tuberculous. I can remember several instances in which a process that began without doubt as a gonorrhœa proved later to be secondary to a renal tuberculosis.

Family history does not seem important. The history, however, of some previous tuberculous trouble, and especially the history of a chronic joint lesion, should make us suspect tuberculosis in a patient who later shows a causeless urinary suppuration. Of the cases I have seen, very few have shown an active pulmonary tuberculosis.

In advanced cases one will often see a marked degree of emaciation. In early cases, however, the patients will usually be in good flesh, and may even be distinctly above the average weight.

In a good many cases the history will lead us to suspect tuberculosis; in a few it will not. The question as to whether a given case is tuberculous or not will have to be definitely settled by a study of the urine. The urine for study should be aseptically drawn and kept free from contamination. When in the sediment of such a urine we find considerable pus, but no organisms, we are justified in strongly suspecting tuberculosis. This depends on the absence of a mixed infection, and two-thirds of my cases have not shown a mixed infection, in spite, in some instances, of a good deal of previous bladder treatment. More definite, in fact, I believe, absolutely diagnostic is the finding of "acid fast" bacilli in a catheter urine. In mixed infections, and in cases where there are very few organisms, we have to depend for our diagnosis upon guinea pig inoculations. The development of tuberculosis in animals injected with the uncontaminated urinary sediment is definite proof of the tuberculous nature of the trouble.

When one finds tubercle bacilli in the purulent urine of a patient presenting the symptoms of a cystitis, I feel that he can practically make a definite diagnosis of renal tuberculosis. The cases in which a tuberculosis of the bladder exists, except as it is secondary to a renal tuberculosis, are extremely rare, almost unheard of in women. In men this condition is occasionally seen, secondary to a tuberculosis of the prostate. The widespread belief in the frequency of bladder tuberculosis as a separate and unrelated condition can of course be explained by the marked bladder symptoms that most cases of renal tuberculosis show, in conjunction, in a large proportion of cases, with a total absence of signs pointing to the kidneys.

If now we have determined that tubercle bacilli are present in the urine of a patient complaining of cystitis, and have drawn the justifiable conclusion that we probably have to do with a renal tuberculosis, we still have to determine the location and extent of the process. In this we are often helped by the lesions of the ureter and bladder that accompany a large proportion of cases of renal tuberculosis. They offer us valuable corroborative evidence as to the nature of the process and often definite evidence as to its location. The common and characteristic change in the ureters, which I have found in 45% of my cases, is a definite tuberculous infiltration of its walls. This thickening of the ureters has, I believe, great diagnostic value. In fact, it seems to me almost pathognomonic of the disease. In women it is often easily recognized by the vagina. Unfortunately, I have not been able to recognize this thickening by rectum in men, even in cases where at operation I have found that it existed. The uretric orifice itself, as seen by the cystoscope, shows nothing that one does not occasionally see in other conditions. It may be red, œdematous, present little ulcerations about its orifice, or it may be excavated, or, due to the infiltration of the ureter walls, the bladder at the point of entrance of the ureter may be pulled out into a funnel-shaped process. While the lesions of the ureter itself may not be characteristic of tuberculosis, a diseased ureter in a case where we have found tubercle bacilli in the urine serves very definitely to locate the diseased kidney.

Secondary bladder involvement occurs in most cases of renal tuberculosis. It was present to a certain degree in 90% of my cases. In the case where it was absent the diseased kidney was shut off from the bladder by an occluded ureter. We have clinically two sorts of bladder involvement in cases of renal tuberculosis, one a definite tuberculosis invasion, the other either an irritation due to

the presence of tuberculous products or a definite cystitis due to a secondary infection. The bladder involvement may be recognized as tuberculous only when one finds typical tubercles, though certain sorts of ulceration are very suggestive. When the tuberculous process is advanced, the bladder shows nothing to distinguish it from other severe infections. When one has the typical bladder involvement, one sees little sub-mucous tubercles about the mouth of a ureter, each tubercle surrounded by a red areola. When they are characteristic and limited to the area about one ureter, the cystoscopic examination may not only tell us the nature of the process but may point definitely to which side is involved. In most cases, however, the mere cystoscopic examination of the bladder is not sufficient to give us all the information we need in cases of renal tuberculosis.

It is essential that we get the urine from each kidney separately, and to do this I depend on catheterization of the ureters. There are other methods, but I believe this is the most accurate one at our disposal. In this procedure, I believe, contrary to the opinion we often hear expressed, that there is little or no danger in catheterizating the ureter on the well side if this is carefully done. In fact, if I have anything to indicate which is the diseased side, I by preference catheterize the well side. I have found that catheterizing the diseased side is much more likely to cause an upset, with rise of temperature, than catheterizing the well side. Then, too, I believe it is quite possible to perforate a thickened tuberculous ureter. Having passed a ureter catheter into the well side, one can usually collect the urine from the diseased side very satisfactorily by means of a catheter placed in the bladder. One can of course catheterize the diseased ureter, if it seems best to do so, and I occasionally find it desirable. There are a number of points of considerable interest that bear especially on the catheteriza-

tion of the ureters in tuberculosis, but they do not properly come within the scope of this paper.

Once one has secured the separate kidney urines their examination, microscopic and chemical, will put one in possession of definite knowledge regarding the location and extent of the disease and allow one to decide as to what should be advised in the way of treatment.

The diagnosis of the latent cases of renal tuberculosis, the ones in which there is a focus in the parenchyma that has not as yet opened into the pelvis, is a difficult matter. A positive tuberculin reaction, the presence of a clear polyuria on one side, with an enlarged kidney on the same side, have enabled an occasional observer to recognize the condition.

In the cases of closed tuberculous pyonephrosis, due to obliteration of a ureter, we may have, as in the latent cases, a clear urine, but with the history of a previous pyuria. More often there is intermittent pyuria; the tuberlin reaction is present, and there is usually a mass in the kidney region. A thickened ureter is helpful in the recognition of these cases, as is the presence of vague shadows in the kidney region, due to old calcified areas. The condition is usually not difficult to recognize. Neither of these types of renal tuberculosis is common when compared with the type that communicates freely with the bladder.

The points I would especially call to your attention in the diagnosis of renal tuberculosis are these: In early cases, and sometimes even in advanced cases, almost all the symptoms point to the bladder rather than to the kidney. There will be little in the urine to suggest kidney involvement, except perhaps a larger amount of albumin than one usually sees in cases of cystitis. The finding of tubercle bacilli in the urine means in a very large proportion of cases the presence of a renal tuberculosis. On physical examination evidence of kidney involvement will be lacking in many cases. In some cases, where evidence of kidney involve-

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ment is present, it may be misleading and cast suspicion on the well side rather than the diseased side. The diagnosis ought always to be confirmed by catheterization of one or both ureters. If one ureter only is catheterized, it should be the well one, since a knowledge of what the good or better kidney is doing is absolutely essential for successful treatment.

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ARTICLE XVIII.

TREATMENT OF RENAL TUBERCULOSIS.

By PAUL THORNDIKE, M.D.
of Boston.

READ JUNE 13, 1911.

In the few moments at my disposal, it is not possible to more than indicate the present status of our knowledge and beliefs concerning the treatment of tuberculosis of the kidney. The patients, as they come to us at hospital or consulting room for treatment, may perhaps be classified, for convenience of consideration, into three groups. First, those beginning cases where the knowledge of early symptoms gained during the last few years has led to a suspicion of the trouble, and where this suspicion has been readily converted to a certain diagnosis by modern methods which Dr. Chute has laid before you this afternoon. Second, those cases where the disease is definitely developed in one kidney, with marked destruction of the organ, much pus in the urine and, perhaps, a mass in the loin, large enough to palpate. Third, those far advanced cases where the disease is obviously of long duration, where other parts of the body are obviously involved, and where the renal disease is only one element, more or less important, as the case may be, in the general condition of the patient.

Class I. The early cases. It is concerning these patients that the greatest interest lies and the greatet difference of opinion seems to exist—the patients who have considerable

bladder irritation, perhaps a little congestion and distortion of one ureteric orifice, a little pus in the corresponding urine, and the diagnosis completed by the demonstration of bacilli in the urine. No constitutional symptoms; no pain aside from bladder discomfort; no tenderness; nothing is clinically present but the vesical uneasiness. What are we to do? Are we to remove that kidney because we know it to have the beginnings of disease, or are we to mark time and treat the patient with the same careful hygiene, climate selection, etc., as if it were the same type and extent of disease at the apex of a lung? The writer has definitely in mind three or four patients of this sort seen in the early years of his practice; patients who were treated without operation—two because they refused it and the others because they were advised to temporize. They all recovered from their symptoms, remained comfortable, and are now, after a dozen or more years, clinically well; but in the two cases in which the urines could be followed there are still tubercle bacilli demonstrable, and in one of the cases, a clergyman in California, this has been repeatedly and constantly the case, as shown by many examinations during these later years of physical well-being. We are all familiar with the wonderful results of nephrectomy in patients with tuberculous disease of the kidney, and it seems to be pretty well proven that not only do these patients recover their physical health, but that in a large percentage of them the bacilli disappear from the urine; so that if such disappearance is to be made the basis of our judgment as to the quality of the final result, we must decide in favor of operation rather than hygiene as our line of therapeutic endeavor. The writer has never yet operated upon a tuberculous kidney without being glad of it and without finding more disease than he expected to find and more than could be inferred from obtainable evidence, and he believes that operation is indicated in the class of patients under consideration, which

have been called beginning cases, for want of a better term, unless for some special and unusual reason pertaining to the individual patient involved.

Class II. The patients with well-marked disease in one kidney and in whom both cystoscope and urine examination give ample evidence of well-developed disease. There is little room for argument or discussion as to the treatment of these patients. The record of nephrectomy with its wonderful results and its many apparently complete cures stands for itself, and there is no brighter page of modern surgical achievement than that filled with the results of this operation in the treatment of tuberculosis of the kidney.

Class III. The patients with advanced disease, both in the kidney and other parts of the body. Patients suffering with pretty generally disseminated tuberculosis are obviously those who do not fall within the scope of any but palliative surgery; but the field for palliation is a large one, and the writer believes that many, even of these patients, may be treated by nephrectomy for palliative purposes. For example: one of his own recent cases was a young man with well-marked disease in both lungs, a spinal caries and tuberculosis of the right kidney, ureter and bladder. The other kidney was apparently well and the patient was having no discomfort except from his bladder. This was a source of such exhausting pain and irritation that it dominated his life and made relief imperative. A nephrectomy was done, and although the patient's condition precluded the removal of the whole of the diseased ureter as well as the kidney, his symptoms have been completely relieved; he has gained many pounds and is doing wholesome out-of-door work with his sinus and his bladder nearly healed. There are very many patients who, although quite beyond the realm of curative surgery, are yet patients so urgently in need of relief from symptoms caused directly by the renal portion of their infection that nephrectomy is as clear an indication as

though it aimed at the complete removal of all existing disease. Whether it should ever be done under these circumstances, and with coincident beginning disease in the other kidney, is a question the writer is not prepared to answer, but such operations must certainly be rarely wise.

As for *palliative operations*, other than a complete nephrectomy, a quotation from a paper by Dr. C. L. Gibson, of St. Luke's Hospital, New York, seems appropriate. He says: "The outlook for operative treatment of tuberculosis other than radical, that is, primary nephrectomy, is very discouraging. Many inexperienced operators, and some who should know better, drain the tuberculous kidney with a marked pyelitis or a complicating perinephratic abscess in the erroneous belief that the apparently insurmountable obstacle to a primary nephrectomy will be favorably influenced by drainage; whereas this drainage does not in any way improve the underlying disease and therefore cannot be expected to reduce the adhesions due to the original process. Furthermore, it opens up new avenues for infection and aggravates thereby the obstacles to a later nephrectomy."

Involvement of the ureter and bladder. We have already been told this afternoon of the line of march of renal tuberculosis down the ureter and into the bladder, so that involvement of these structures is both natural and common, and it is seldom that one sees a patient so early in the development of his disease that some evidence of it does not show in the bladder, at least at the corresponding ureteric orifice, if not with greater dissemination. This seems perhaps discouraging from the point of view of curative surgery, but the issue does not prove so, and nature shows herself to be abundantly able to take care of portions of disease left behind, provided always the kidney, the central focus of infection, be removed. So, in operating upon tuberculous kidneys, while one should make every effort to remove

the disease as thoroughly as possible, yet he may feel confident that involvement of portions of the ureter left behind and the invaded portions of the bladder will take care of themselves and gradually heal. This is the rule, and takes place often in most extreme and fully developed cases. This fact and the fact that involvement of the second kidney is not very common, and when it does occur usually comes late in the progress of the disease, gives us our best guaranty that we may hope really to cure some by nephrectomy.

Tuberculin. Our knowledge of the facts regarding the use of tuberculin as a means of cure in lesions of the kidney is by no means extensive enough at the present time; and we can only draw our own inferences and base our opinions and conclusions upon the character of the conditions involved. If one looks at the pathological condition in such kidneys at the time of operation, even of early operation, and we all operate nowadays as early as possible, it is hard to imagine how tuberculosis can control such formidable foci of disease in any complete or permanent way, nor how it can ever come to take the place of nephrectomy; but it nevertheless has a place and a real place in the therapeutics of renal tuberculosis after operation, as complete as possible, has been performed. The writer believes that most surgeons at the present time will agree that the use of tuberculin is of real assistance in promoting the healing of sinuses after operation and the hastening of the quieting-down process in infected ureter stumps and bladder ulcers. It is, then, useful chiefly as an adjuvant and not as the sole resource at our command.

Technic. In closing, there are two or three points in the technic of nephrectomy for tuberculosis which may properly be mentioned, although there is little to say, except that operations should be as thorough as possible, and all possible care should be taken to provide no new avenues of infection. This latter statement is especially to be remem-

bered in dealing with patients in whom the diagnosis is made early. Although operation is often done when not only is the disease perfectly apparent as soon as the kidney is exposed to inspection and palpation, but even after perirenal involvement has taken place, yet there are some operations, and they are ever becoming greater in number, performed before the disease has penetrated the renal capsule, and before it has indicated its presence to inspection or palpation. In these cases, it is necessary to open the kidney before removing it, and such incisions should be done with the greatest care taken to prevent contamination of the surrounding parts uninfected up to that time. The same precautions should be taken when the ureter is divided, and its stump should always be cauterized, perhaps with carbolic acid and alcohol, whether presenting obvious indication of disease or not. As for the amount of ureter to be removed, there seems to be considerable difference of opinion. The writer believes that while all these operations should be as thoroughly done as possible, and while the complete removal of the kidney is essential to a successful result, the complete removal of the ureter is by no means so; and while it may often be removed without subjecting the patient to undue risk of life, in cases of doubt the lesser procedure should always be selected, remembering the fact that infected stumps usually subside and heal when the infecting center is removed. If complete removal of the ureter is to be attempted, it is well to remember that the long incisions, which expose the whole track of the ureter, are not always necessary or wise. The ureter lies adherent to the under surface of the peritoneum, from which it can usually be stripped without much difficulty, even when considerably diseased, and this may be accomplished from below after the removal of the kidney through the loin, through the incision advocated by C. L. Gibson for approaching the bladder end of the ureter, or through some other of the several similar methods of approach.

SYMPOSIUM ON RENAL TUBERCULOSIS.

ARTICLE XIX.

PROGNOSIS OF RENAL TUBERCULOSIS, WITH AND WITHOUT OPERATION.

By HORACE BINNEY, M.D.
OF BOSTON.

READ JUNE 13, 1911.

Among the many lines of advance in the past quarter of a century which modern medicine can show, there is certainly no more striking one than in the special field of renal tuberculosis. This progress is the result of our increased knowledge on two particular points: first, the resisting power of the body to tuberculosis under ideal conditions of nutrition, hygiene and climate; second, the existence of the disease in its earliest stage as a localized process—for practical purposes primary in the kidney, and strictly so as regards the genito-urinary tract. Modern treatment, based on these principles, has changed the character of the disease so that we recognize, today, that at least in its earliest stages, medical and hygienic treatment can sometimes delay its progress for months, if not years, while surgical removal of the primary focus by nephrectomy will often be the means of permanent cure.

These remarks are axiomatic as regards the prognosis of renal tuberculosis in general. But when we attempt an estimation of the prognosis in any particular case, we find many factors entering into it. Of these, the more important are the extent of the process in one or both kidneys, the existence of tuberculosis in other organs than the kidney,

and in cases of unilateral disease the question of the integrity of the other kidney; that is, the presence or absence of nephritis is of great importance. Finally, the age, general condition, and circumstances of the patient must be considered.

With the exception of the first, namely, the extent of the renal process, these factors affect the prognosis equally whether medical or surgical treatment is carried out. Thus we know that the better the patient's general condition and circumstances and the more free he is from tuberculosis in other organs than the kidney, the greater his chances for improvement under any form of treatment. We also know that while a kidney which is only mildly irritated by the tubercular toxin secreted by its diseased fellow will, on removal of the cause of irritation, rapidly recover and do the work necessary to sustain life; on the other hand, if it has become the seat of an advanced parenchymatous nephritis, it will be unable in many cases to sustain life after its fellow has undergone further destruction or has been removed by nephrectomy. Again, it is well known that children possess a greater recuperative power in this disease than do adults. These factors have an important bearing on the prognosis in general, but are less concerned with the question at issue. The comparative prognosis under medical and surgical treatment depends more directly upon the extent of the renal tubercular process. Therefore we may waive further discussion of these more remote factors, and consider more closely the various degrees of the renal infection.

For this purpose the cases may be roughly classified as follows:

1. Incipient unilateral tuberculosis.

2. More advanced unilateral disease, in which the ureter and bladder may be more or less involved.

3. Bilateral renal disease, which of course includes, practically always, disease below the kidneys also.

Medical Treatment. What is the prognosis under medical treatment of these three groups?

1. Early cases. It is almost universally admitted today that tuberculosis will advance under medical and hygienic treatment even if wholly confined to a single kidney. I do not here include the early cases in children treated with tuberculin, of which I shall speak later.

The apparent recoveries under hygienic treatment which are occasionally reported are, in the vast majority, lacking in sufficient evidence on which a cure can be justly claimed. The frequent observation of kidneys more or less completely destroyed, with obliteration of the ureter, are on the other hand so well known that further discussion of this point is superfluous.

2. More advanced, and (3) bilateral cases. If medical and hygienic treatment offers so little hope of cure in the earlier cases, it offers less here. The most striking statistics on this point are those of Blum[1], who observed thirty-two cases under medical treatment for six months or longer. Of these there were two cases alive, twenty-four had died, and the condition of the remaining six was not known at the time of reporting. This gives a mortality of at least 75% under medical treatment.

The average duration of life from the first appearances of involvement of the urinary tract was two years and three months. Death occurred from a variety of manifestations of the disease; the most common was tubercular cachexia and septicemia; less frequent causes, occurring in about equal numbers, were miliary tuberculosis, tubercular meningitis, tubercular peritonitis, and finally, anuria and uremia.

Prognosis under Tuberculin Treatment. Being a part of the medical treatment, the prognosis under tuberculin as a means of combating the disease is properly considered here.

It has won many adherents in the past few years, and the literature contains a goodly number of isolated case reports. They are open to the same objections as so many of the re-

ports of spontaneous cure, or cure by medical and hygienic treatment, in that the period of observation is short, and the absence of tubercle bacilli from the urine is not proved. The bare facts of gain in weight and improvement of symptoms are not positive evidence of cure.

A recent summary of all the reported cases treated with tuberculin, collected and published by Gardner[3], showed a total of eighty-three cases. Of these, eighteen were claimed to be cured, thirty benefited, twenty-five not improved. The author points out, however, that the claim for cure in these cases is, with few exceptions, made on the fact of the patient appearing well.

While remarkable improvement in some cases follows the use of tuberculin, plus hygienic measures, yet there are abundant cases reported where tuberculin has failed and subsequent nephrectomy has demonstrated active tuberculosis of the organ. It seems impossible, therefore, to offer at present prognosis of certain cure in any case under tuberculin treatment.

Prognosis under Surgical Treatment. The abundant statistics of the past few years demonstrating the excellent results of nephrectomy in renal tuberculosis are too familiar to need repetition here. It is enough to mention that in the hands of some operators the operative mortality in a series of 50 or more nephrectomies for unilateral disease is as low as 3% (Kümmell[5]). Dollinger[6] reports 35 cases without a death.

Early Cases. While modern methods of diagnosis are giving the cases to the surgeon in the earliest stages with increasing frequency, there are, as yet, no extensive statistics showing how large the percentage of cures is. Kümmell specifies 12 of his cases as "early," and stated that all were "entirely well." Although evidence of symptomatic cure is ample in the large majority of reported cases, I think that positive proof, namely, by the guinea-pig test, is necessary before the medical public can be expected to

accord their unqualified approval of nephrectomy as a routine treatment.

In this connection, Lichtenstern's[7] studies are of interest. He subjected the urines in 17 nephrectomized patients to repeated inoculations, and in 10 cases the tests in a reasonable time became negative. Those with considerable bladder involvement were slower in becoming bacilli-free than cases with little or no bladder lesions.

The remaining 7, while symtomatically well or improved, still showed bacilli in the urine after several months.

It will be seen, then, that in stating the prognosis for early and moderately advanced renal tuberculosis, the two groups must be considered together. Three years ago, Watson,[8] from collected statistics in over 300 cases, estimated the operative mortality to be from 7% to 10%. Doubtless this figure is somewhat high to-day, considering the more widespread use of modern methods of diagnosis.

As to duration of life after nephrectomy, the literature of the last five or six years abounds in reports of cases well, after several years. Exact statistics, however, are still lacking, the period of observation being insufficient in many reports. Israel's[9] results are, however, worth noting. 75% of his series of 97 nephrectomies were known to be living at the time of report, the period of observations being from a few months to fifteen years.

He states that the bacilli disappeared from the urine in 93%, after nephrectomy. A conservative estimate as to the frequency of permanent cures of early and moderately advanced cases would be from 60% to 75%.

Regarding the tubercular process in the bladder when present, the marked tendency for this to heal, often within a few months after the nephrectomy, is a fact well established by the cystoscopic reports of many observers.

Bilateral Cases. This is naturally an unfavorable field for surgery, since in the average case the process in the better kidney will have progressed too far. Undoubtedly

much of the mortality of early operators was due to failure—with the insufficient methods of diagnosis at hand—to recognize the extent of the damage already present in the better kidney.

Recent progress in this particular, with consequent lessening of mortality, is making this field a more hopeful one; but, since reports are as yet incomplete, or methods of diagnosis in some cases untrustworthy, the statistics of nephrectomy in bilateral disease are not reliable. In determining a bilateral condition, catheterization of both ureters is the only means of obtaining positive evidence before operation; and this method has not always been employed by surgeons reporting cases of "bilateral" disease.

A study of reported cases indicates that a slight tubercular process in one kidney does not prohibit nephrectomy of an extensively diseased other kidney: particularly if it is in a condition of pyonephrosis or is a source of systemic poisoning. In such cases, since the progress of the disease may be delayed, and irritation is removed, a temporary improvement in health may be expected.

Other Methods. Since nephrotomy does not aim at removal of the disease but merely at treatment of a complication, while partial resection of a kidney has very few advocates, the prognosis under these operations has not been included in this study.

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DISCUSSION OF THE PRECEDING FOUR PAPERS.

DR. H. W. BEAL, Worcester: I have been much interested in these papers, but they seem to have left very little for discussion. There is one point in regard to renal tuberculosis that is of primary importance, and in early cases of some difficulty, that is, the diagnosis. The diagnosis of renal tuberculosis is inseparably connected with the use of the cystoscope.

It is interesting to note what advantages have come to the surgeon in recent years in his operating, by the use of the cystoscope. Only a few years ago the surgeon was compelled to operate with the information obtained by palpation and examination of the urine; rough methods, which sometimes led to a correct procedure, but often led to operative trouble. Despite the accurate information that may usually be obtained from the cystoscope and ureteral catheter, it is surprising to see some good surgeons to-day operating without the advantage of a cystoscopic diagnosis. One argument that has been advanced against devoting a few days to a cystoscopic diagnosis, is that of hospital economy; it is said that the patient should be operated on within a day or two after his arrival in the hospital, and that delay for cystoscopic examination means expense that most hospitals can ill afford.

This principle of treatment is not only unscientific, but is unfair to the patient, who is entitled to services that will make as complete a diagnosis as possible before the operation.

Considered from an economic point of view only, probably the days spent in hospital would be much less for the patient on whom an accurate cystoscopic examination had been made, than for one who had been operated on after a diagnosis had been made by rough methods, on account of the possible necessity of a second operation in the latter case.

The surgeon who operates on renal cases without all the information obtainable by the cystoscope is taking unnecessary risks, and is not giving his patient the best chance for recovery.

Recently, in a city west of Massachusetts, I saw one of

the best surgeons in this country operating on a kidney. A radiograph had been taken, and showed the shadow of a stone. The operation was for the removal of this stone, but the surgeon unexpectedly found a large pyonephrosis containing a small stone. There had been no cystoscopic examination, and the absence of information in regard to the condition of the other kidney made a nephrectomy, the operation of choice, impossible. The kidney was drained, and the patient was to await a complete diagnosis before the second operation.

This case is an example, neither of the surgeon's best efforts, of most consideration for the patient, nor of hospital economy. It was interesting to hear in the amphitheatre, after this operation, numerous comments on the failure of the surgeon to obtain a cystoscopic diagnosis before operating.

The use of the cystoscope has greatly increased in the past few years, and I think all surgeons are beginning to realize that it is an absolute necessity for correct diagnosis.

I show you lantern slides illustrating the extent of bladder ulcerations, and the kidney condition, in two cases of renal tuberculosis in which nephrectomy was done.

In both, the symptoms were of six months duration.

The first, a woman of 37, with ulcerations occupying an angle of 30° up from the ureteral orifice, healed without treatment in six months. The second, a woman of 54, with ulcerations covering the whole of the posterior wall, healed without treatment in thirty-three months.

Dr. R. F. O'Neil, Boston: I wish to congratulate the readers upon a very interesting and complete set of papers, which as Dr. Beal has said, leave very little for discussion.

I would like, however, to emphasize one or two points. One is the danger of placing much importance upon pain in the kidney region as pointing to the diseased organ in renal tuberculosis; another is the necessity of employing ureter catheterization in making a diagnosis. These points are well illustrated by a case I saw at the Dispensary, in which the diagnosis of double renal tuberculosis had been made upon bilateral pain, palpable kidneys, turbid urine containing tubercle bacilli and marked bladder symptoms. A simple cystoscopic examination had been made, but the ureters

had not been catheterized; a more extensive examination showed the lesion to be limited to the right side. I think this is the same case that Dr. Chute referred to in his paper.

One more point is, that tubercle bacilli may be found in the urine of patients with tuberculosis of other organs and no involvement of the kidneys. This might lead to an incorrect diagnosis were it not remembered that in tuberculosis of the urinary tract, signs of inflammation as pus and sometimes blood are always present.

It is remarkable the improvement that takes place in these cases after nephrectomy, not only in the urinary tract, but in other tuberculous foci if present, even in some of the worst cases. There should be no question but that early nephrectomy in unilateral cases, and in some biliteral cases, is the treatment to be carried out.

DR. CABOT: In the first place I want to comment on the statement made by Dr. O'Neil, that the passing of bacilli in the urine does not mean that the patient has tuberculosis of the kidney. The evidence of inflammation must count; otherwise the finding of the bacilli is of no use.

The tubercle bacilli as found in the urine may offer a possible source of error.

I would lay more stress than did Dr. Truesdale upon the use of the guinea pig; the guinea pig will do the work without any question, and I believe that the inoculation of the guinea pig will prove the matter perfectly and I believe should be used as a routine procedure—should be used even if he does cost 50 cents.

Dr. Truesdale spoke of the thickening of the ureter as pathognomonic. I have seen several such cases, and in every case it was not tuberculosis of the kidney.

I would lay great stress upon the renal condition; the operation is going to depend upon the ability of the sound kidney, not only to do its own work, but also the work of the other one. Tubercle bacilli having been found coming from one kidney, if the function of the other kidney makes it appear able to sustain life, the sick kidney should be removed.

In bilateral cases I think it would be justifiable, if we are of the belief that the body has gotten up steam to kill the bacilli in both kidneys, to remove the worse kidney.

In this way you may be able to deal with the process, but I believe that evidence is wanting to prove that tuberculosis of the kidney is ever cured.

I believe tuberculin is of great value in helping out cases after operation; I do not believe that otherwise it is of any benefit, but the vast majority of cases have been benefited as to their lesions and symptoms.

Lesions of the ureter take place at the two ends of the ureter; only in the advanced cases is the whole ureter involved. You remove the upper portion of the ureter with the kidney; to remove the next four or five inches is, I think, a waste of time; but if you are going to be thorough, take out the whole ureter and the bladder orifice.

I do not agree fully with Dr. Beal; I believe the invasion resulted from the lowered vitality.

Dr. Binney: I would say a word in regard to what Dr. Cabot has so thoroughly covered, the so-called spontaneous cure. In reviewing the literature I have found reports of many cases where this was claimed. In every one where pathological examination was made there was evidence of the kidney being destroyed, although usually no active bacilli were present. One German observer, in a list of seventy cases, found but one where the process had become latent. On the other hand one French observer reports a case where diagnosis of tuberculosis of the kidney had been made twenty years before operation. The patient was a strong resistant person in whom a tumor in the kidney region finally developed. A large pyonephrotic kidney was removed, in the tissue of which tubercles were found and evidence of living active bacilli.

SYMPOSIUM ON SALVARSAN FROM THE CLINICAL STANDPOINT.

ARTICLE XX.

INTRODUCTION.

By HARVEY P. TOWLE, M.D.
OF BOSTON.

READ JUNE 13, 1911.

As you know, Salvarsan (Ehrlich's "606") has been widely discussed. The medical literature has been overrunning with reports of the drug, its action and its indications. Nevertheless, we are still in doubt as to very many of the facts concerning it. It is still under fire. It became very evident during the recent discussion of this subject at the Massachusetts General Hospital, that the average man was doing his reading for himself. The questions which followed that meeting were all variations of "What do you know about this drug?"

At this meeting the speakers have been asked to give their own personal experiences. It was by no means planned that we were to settle the question of salvarsan, or its place in therapeutics, but to give you ideas, practical points in handling, indications for its use, something about doses, the accidents and mishaps attending its use and ways of avoiding them; all as illustrated by personal experience.

In order to make the subject, or the symposium, complete, as you will notice by the program, we have begun with the chemistry of the drug, which takes in, of course, Ehrlich's idea of chemotherapy, then follows the parasitology of syphilis.

Unfortunately the paper upon the Wasserman Reaction cannot be given, owing to Dr. Mooers' death a few days ago.

We have also planned to give you a practical demonstration of the making of the solution, and finally Dr. Cabot, Dr. Pollitzer and Dr. Post have been good enough to come here and give their experiences, each in the light of his previous large experience with mercury.

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SYMPOSIUM ON SALVARSAN FROM THE CLINICAL STANDPOINT.

ARTICLE XXI.

THE PARASITOLOGY OF SYPHILIS.

By HAROLD C. ERNST, M.D.
OF JAMAICA PLAIN.

READ JUNE 13, 1911.

THE etiology of Syphilis has of course been the subject of much attention and of extensive research for many years. With the advent of the methods of Bacteriology many investigators took up its study from this new point of view and hasty and ill-considered assertions were made as to bacteria found in the lesions of the disease. For some time special attention was paid to the statements of Lustgarten (1884), who claimed an etiological relationship to the disease for an acidfast bacillus three to four microns in length, which occurred in small bundles in the cells or free in the intercellular spaces. Other observers were unable to confirm his results—so far as finding this bacillus in the tissues, and the conclusion seems to be justifiable, that Lustgarton's bacillus was in fact the smegma bacillus—at that time not recognized, but soon demonstrated to be of common occurrence in the smegma and in parts contaminated by it.

In 1905 Schaudinn and Hoffman (Deut. Med. Woch. May 4, 1905, p. 711) described Spirochaeta pa lida and Spirochaeta refringens as occurring in all the lesions of syphilis. They were at first inclined to think that both these organisms were concerned in the production of the disease, but later study served to convince them that S.

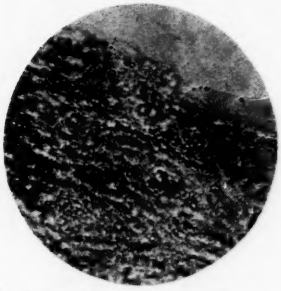

FIG. 1. Treponema pallidum. Section of liver of child. Congenital syphilis.

FIG. 2. Treponema pallidum. Section of liver of child. Congenital syphilis.

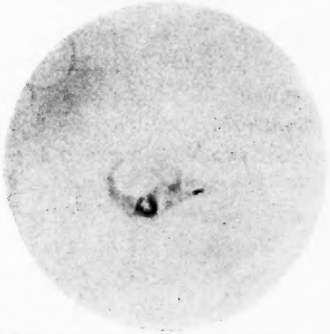

FIG. 3. Trypanosoma equiperdum. Cover-glass preparation from tissues of horse.

All magnified 1,000 diameters. The sections from Dr. S. B. Wolbach; the cover-glass preparation from Dr. E. S. Tobey. All stained with Giemsa.

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refringens is merely a common accompaniment of the lesions, and is not concerned in their causation. All observations since that time have served to confirm the belief that S. pallida is the chief—if not the only—cause of this infection, and this in spite of the fact that thus far pure cultures have not been obtained, and inoculation experiments have been successfully carried out only with material from syphilitic lesions containing the organism. In default of pure cultures collateral evidence—such as bactericidal or agglutination tests—are lacking.

The organism has been demonstrated however in practically all stages of syphilitic lesions—even the tertiary—as well as in the tissues of monkeys and other animals inoculated with syphilitic material. In some of these inoculated animals, too, the serum has presented the Wassermann reaction.

The terminology applied to the organism has varied—this as the result of a wide-spread discussion as to the place to be assigned to spirochaetae. S. pallida—according to Schaudinn—differs from others, often accompanying it in syphilis (S. refringens, S. plicatilis), and from the spirochaetes of Vincent's angina, of ulcerated cancers and others, in that it has no trace of an undulating membrane, is circular—not flattened—on transverse section, and has a long flagellum at each end. Inasmuch as true spirochaetes have at least traces of an undulating membrane, are always somewhat flattened, and have no flagella, the so-called S. pallida should be taken out of this genus. Schaudinn proposed the name Spironema pallida (*σπεῖρα*, a coil, and *νῆμα*, a thread), but this name was abandoned almost at once for it was found to be already claimed, and the name Treponema pallidum (*τρέπειν*, turn, *νῆμα*, thread) is the one properly applied to the organism at present, it thus forming the leading member of a new genus. It varies from four to fourteen microns in length, and is but one fourth of a micron in thickness. In fresh preparations in

salt solution it is very actively motile, rotating, moving to and fro, and making flexing movements which are easily seen. These characteristics separate it from Spirillum (diminutive of Latin spira, a coil) which are spiral cells, rigid, but with flagella which propel them to and fro. They also separate it from the spirochaetes (*σπεῖρα*, a coil, and *χαίτη*, a bristle), which are long slender threads usually with narrow spiral windings, with active to and fro movements, and ability to bend in varying directions.

Treponema pallidum has usually six to eight curves, and is not very difficult to demonstrate in the fresh condition if a good dark ground illuminating apparatus be employed. Its movements under such conditions make it fairly easy to differentiate. Stained preparations can be secured by making cover glass smear preparations as thin as possible from tissue scrapings. Harden in absolute alcohol for ten minutes, or in osmic acid vapor for a few seconds and stain with Giemsa or Borrel blue. Sections of the organs in syphilis—from adults or cases of congenital disease—often show the organism in enormous numbers.

Treponema pallidum being placed in a new genus and removed from among the spirochaetes, the relationship between it and the trypanosomes has become less probable than before. It is, however, interesting to note the discussion that has gone on in this direction, for later evidence may be brought forward connecting the two. If this should happen certain histological resemblances between Dourine (Mal du coit) and syphilis would be of increased interest by such grouping of the etiological factors. Dourine is of course a disease produced by a true trypanosome (Tr. equiperdum) but certain of the histological changes—especially of the nerves—are allied to those seen in syphilis, and it would be interesting if these two diseases should come to be grouped together as has been done in so many other cases in recent years—by better knowledge of the causal agent.

SYMPOSIUM ON SALVARSAN FROM THE CLINICAL STANDPOINT.

ARTICLE XXII.

THE CHEMISTRY OF SALVARSAN.

By WILLIAM F. BOOS, M.D., Ph.D.
OF JAMAICA PLAIN.

READ JUNE 13, 1911.

DURING the past year a number of medical journals have published papers on the chemistry of salvarsan, that were intended to make the matter clear to the medical practitioner. The treatises on the subject which I have seen show one fault in common—they do not emphasize the close relationship which exists between the immediate forerunners of salvarsan and the mother substance, arsenic acid. This relationship can only be shown by the use of structural formulas.

In the teaching of inorganic chemistry the very important structural formulas are sadly neglected, and many facts which may be learned at a glance from such formulas are never completely mastered by the student, because the empirical formulas so commonly used do not explain "the reason why." I have known trained chemists taught in the old-fashioned way to be ignorant, for example, of the fact that an oxygen acid contains all the hydrogen atoms which are replaceable by a metal (or other basic radical) to form a salt, in the state of OH, a group known as the hydroxyl, or, for short, as the "oxy" group. It seems to me that a knowledge of this fact is essential to an intelligent understanding of the simplest chemical changes.

Let us consider for example four of the common acids, namely, nitric acid, sulphuric acid, phosphoric acid and arsenic acid. The empirical formulas HNO_3, H_2SO_4, H_3PO_4, H_3AsO_4, tell us nothing about the arrangement of the atoms, nor do they explain why nitric acid contains only one replaceable hydrogen atom ($NaNO_3$), while phosphoric

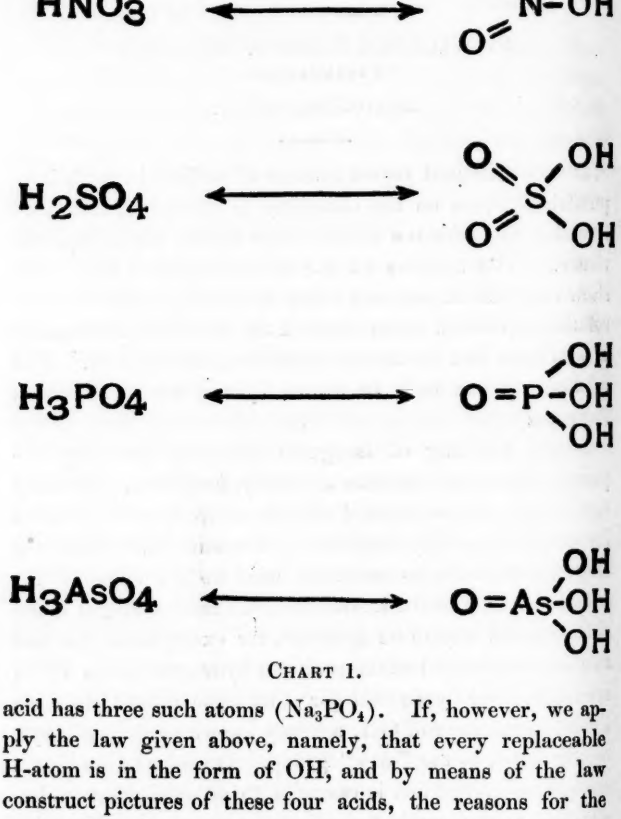


CHART 1.

acid has three such atoms (Na_3PO_4). If, however, we apply the law given above, namely, that every replaceable H-atom is in the form of OH, and by means of the law construct pictures of these four acids, the reasons for the various formulas are at once apparent. Nitric acid contains

only one H atom replaceable by bases to form a salt; therefore it contains one, and only one, OH group. In sulphuric acid there are two replaceable H atoms (Na_2SO_4); this acid must therefore contain two OH groups; phosphoric acid, H_3PO_4, and arsenic acid, H_3AsO_4, both have three replaceable H atoms (Na_3PO_4, Na_3AsO_4); each of these acids must therefore contain three OH groups, as shown by the first chart.

The structural formula of an acid salt such as NaH_2PO_4 contains one ONa group and two OH groups; that of the salt Na_2HPO_4 contains two ONa groups and one OH group; that of the neutral salt Na_3PO_4 contains three ONa groups and no OH groups. The presence of the OH groups in the acid phosphates explains their partial acid character, which is sufficiently pronounced to render their solutions acid to litmus (acid reaction of the urine).

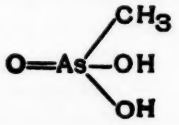
Since oxygen always shows a valence of two and the OH or oxy group a valence of one, it is easy from the structural formula to determine in each case the valence of the non-metal characteristic of the acid. Thus we find that the nitrogen in HNO_3 has a valence of 5, that the sulphur in H_2SO_4 has a valence of 6, that the phosphorus and arsenic in H_3PO_4 and H_3AsO_4, respectively have a valence of 5.

I shall attempt in this paper to show by easy stages how salvarsan may be developed from arsenic acid, using for this purpose the simple structural formulas of some of the landmarks, so to speak, established in the process of investigation.

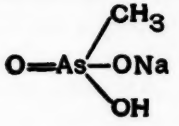
Taking the formula of arsenic acid as shown in Chart 1, it is possible to replace not only the H atom but the entire OH group by a basic radical such as CH_3, the methyl group, for instance (Chart 2). When the entire OH group is replaced in this manner, the fact is indicated by a change in the name of the acid to arsinic acid. The replacement of one oxy group by CH_3 gives us mono-methyl arsinic acid,

commonly called methyl arsinic acid. When two OH groups are replaced by methyl groups, we have di-methyl arsinic acid, and so on.

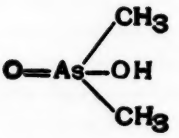
The sodium salt of methyl arsinic acid is well known in medicine under the name of arrhenal. The di-methyl acid is even better known; it is cacodylic acid, the sodium salt of which has recently been highly recommended in this country and France as a specific in syphilis, just as if Ehrlich had not tried it out thoroughly and found it quite inadequate, many years ago. Cacodylic acid and its sodium salt owe their



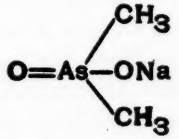
Methyl arsinic acid



Arrhenal



Cacodylic acid



Sodium cacodylate

CHART 2.

action in part, as was shown by Schultz,* to the decomposition products formed in the system. They are not only unreliable in their action, but are apt to cause sudden and serious intoxication from the liberation of arsenic acid. Heffter† found that a part of the acid or of its salt is excreted unchanged in the urine.

Ehrlich was in search of a stable organic derivative of arsenic acid which itself would act as a parasiticide and not

* Schultz, Arch. f. Exp. Path. u. Pharmakol, 11, 131, 1879.
† Heffter, Arch. f. Exp. Path. u. Pharmakol, 46, 230, 1901.

through highly toxic decomposition products formed in the system. He hoped for better results from the benzol derivatives of arsenic acid. One of the first of these which he tried was atoxyl.

Atoxyl was described by Béchamp in 1863; he called the free acid an anilid of arsenic acid. Experimenting with the compound, Ehrlich found that it was easily diazo-

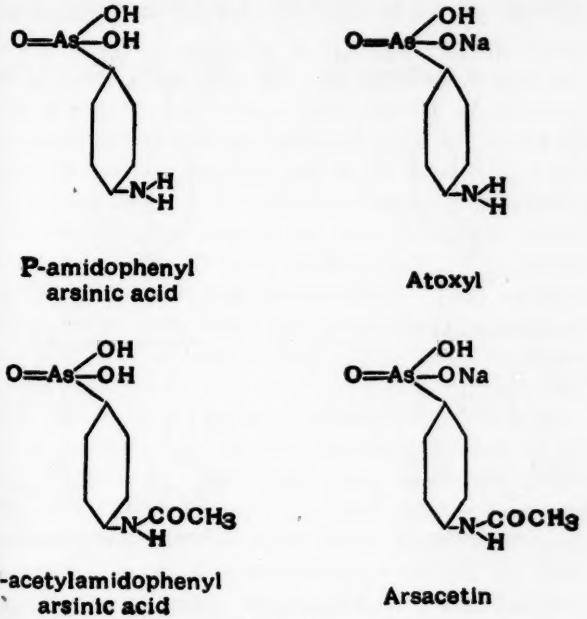


CHART 3.

tized and converted into an azo dye. From this property he reasoned that the substance must contain a free NH_2 or amido group and that it was in truth p-amidophenyl arsinic acid. The compound is derived from arsenic acid by the replacement of one OH group by $C_6H_4\,NH_2$, the amidobenzol or aniline group. For the purpose of simplification

and better to show the structure of the compound, the benzol ring, so-called, is shown in the charts as the conventional hexagon to which the various groups NH_2, OH, etc. are attached at definite fixed points. The point of the hexagon directly opposite the arsenic atom is known as the para (p) position, while the angle of the hexagon nearer by one on either side to the arsenic atom is known as the meta (m) position. It will be seen from Chart 3 that atoxyl is simply

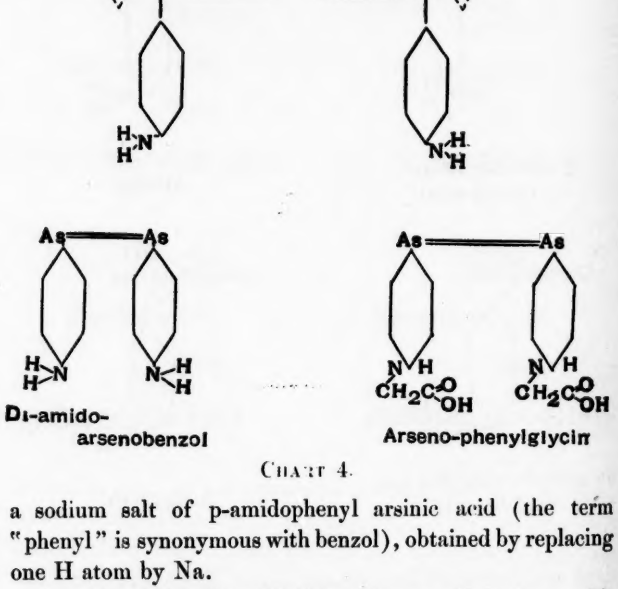


Chart 4.

a sodium salt of p-amidophenyl arsinic acid (the term "phenyl" is synonymous with benzol), obtained by replacing one H atom by Na.

Atoxyl was found by Ehrlich to be an efficient parasiticide, particularly as regards the spirillum of relapsing fever. Its action on the parasite of syphilis was not efficient, atoxyl, moreover, contrary to its name, was found to be very toxic and too dangerous for human therapy.

By the use of acetyl chloride it is possible to replace one

hydrogen atom of each NH_2 group in p-amidophenyl arsinic acid by the acetyl group. The sodium salt of the resulting compound is known as arsacetin. Arsacetin is less toxic than atoxyl, but this substance also is inefficient in its action on the parasites of the syphilis group.

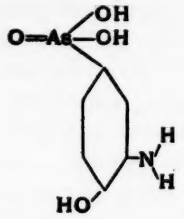
In the course of his extensive experimentation Ehrlich conceived the idea of combining two molecules of p-amidophenyl arsinic acid into one compound by causing the two atoms of arsenic to unite with each other. Chart 4 shows how by means of reduction (H) and dehydration, six molecules of water are split from two molecules of p-amidophenyl arsinic acid, leaving nothing for the two atoms of arsenic to combine with but each other, their valence being reduced to 3. The resulting compound is di-amidoarsenobenzol. By treating this compound with monochloracetic acid one H atom of each amido (NH_2) group may be replaced by the acetic acid radical. From its close relationship to methyl acetic acid or glycocoll the resulting compound is called arseno-phenyl-glycin. This substance Ehrlich found to be very effective against trypanosomes, but its action on the parasite of syphilis was unsatisfactory.

After many experiments with different derivatives of di-amido-arsenobenzol, Ehrich used other compounds in his arsenobenzol condensation. One of these was p-oxy-m-amidophenyl arsinic acid, which differs from the original compound used for the condensation in having one OH group in the para position and one NH_2 group in the meta position (Chart 5). The product of condensation, di-oxy-di-amidoarsenobenzol, number 605 in the series, was found to be very effective against the spirochæta pallida of syphilis.* The compound is, however, very difficultly soluble, and to obviate this fault Ehrlich converted it into its hydrochlorid which is shown in Chart 5 as 606.

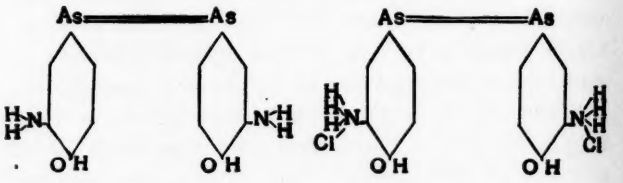
* Hoffmann, "Ueber die Bennenung des Syphiliserregers, etc." Muench. Med. Wochschr. Vol. 58, p. 1796 (1911).

Ehrlich used this compound extensively on rabbits with relapsing fever; later, on rabbits inoculated with syphilis from humans and from one another. His remarkable results with 606 prompted him to recommend this substance for use in human syphilis.

Six hundred and six, or salvarsan as it is now called, is very stable and acts as such rather than through decomposition products. That is why relatively much greater quan-



P-oxy-m-amidophenyl arsinic acid



"605" "606"

CHART 5.

tities of arsenic may be given in form of salvarsan than as cacodylate of sodium or atoxyl. A considerable part of the dose injected is probably bound immediately by the liver, which then permits a small quantity at a time only to pass into the circulation on its way to excretion. In this way long continued action is insured.

It was originally supposed that when salvarsan is given intravenously the entire dose injected is excreted in a

few days. While it is undoubtedly true that more of the drug is excreted within twenty-four hours after intravenous injection than when it is given intra-muscularly, it has been shown on the other hand that the subsequent excretion after intravenous application may continue for weeks. I examined the twenty-four hour urine of one of my patients ten days after giving him an intravenous infusion of .6 gm of salvarsan and I was able to recover a little over one milligram of arsenic. Jesionek* found arsenic in the urine of a patient during the fourth week after the intravenous application of .4 gm salvarsan.

I found the excretion of arsenic in the urine after the intramuscular injection of salvarsan to average .7 milligram a day for the first five or six days, then it gradually fell until at the end of two weeks it was about .3 milligram in twenty-four hours. I think the intramuscular method does not permit enough salvarsan to enter the circulation at any one time to be sufficiently effective against the parasites, it having been shown repeatedly that two intravenous applications will do more than six injections given into the muscle. The intravenous method is therefore, I think, the only method to be used. If it is properly carried out by physicians with extensive experience, it is not only as safe as the intra-muscular method but even safer, because the great number of unavoidoble sloughs which follow the application into the muscle are a very serious menace to the health of the individual. Of course only strictly suitable cases should receive salvarsan, but this is true of either method of application.

* Jesionek, Muench. Med. Wochenschr., 1911, Vol. 58, p. 1169.

SYMPOSIUM ON SALVARSAN.

ARTICLE XXIII.

THE PREPARATION OF SALVARSAN FOR INJECTION.

By Mr. JOSEPH GODSOE,
Pharmacist, Massachusetts General Hospital, Boston.

READ JUNE 13, 1911.

A few suggestions relative to the mixing of Salvarsan or "606" may be of some benefit to the physician who wishes to mix the preparation for patients in his private practice.

The following articles are all that are required for mixing the substance for intramuscular or intravenous administration:

Mortar and pestle (wedgewood, glass or porcelain).
One oz. wide mouth bottle.
Graduated burette with stand.
Cylindrical graduate, 10 cc.
Mixing bottle (ordinary graduated nursing bottle).
Glass beads (or glass rod cut in small pieces).
Glass funnel and filter-paper.
Flask of salt solution, 0.6%.
Bottle of normal sodic hydrate solution.

All apparatus should be sterilized, and the solution should be prepared under aseptic conditions.

It requires from 0.7 to 0.75 cc. normal Na OH solution to dissolve 0.1 gramme of Salvarsan. If the normal Na OH solution cannot be obtained, use a 4% solution. If the 4% solution is used, it may require more or less to dis-

solve Salvarsan than is stated above, because the strength of the Na OH varies considerably.

To make the solution for intramuscular use, place 0.6 grammes of the powder in the mortar, add 4 cc. of salt solution, triturate thoroughly until a clear solultion is obtained. To this solution add 4.5 cc. of normal Na OH solution, using about 1 cc. at a time. Upon the addition of the first cc. of Na OH solution a gelatinous mass will form, which will redissolve when more of the Na OH solution is added. Triturate thoroughly after each addition and add the last 0.5 cc. drop by drop until the solution is perfectly clear. Transfer to the one ounce wide mouth bottle, wash the mortar out with a small quantity of the salt solution, and finally add salt solution up to 16 cc.

For intravenous use:

Place the glass beads and 0.6 grammes of the powder in the mixing bottle, add 15 cc. of salt solution and shake thoroughly until the solution is perfectly clear and free from transparent gelatinous clots. Then add the normal Na OH solution, using the same quantity and method as directed in the former preparation. Then make up with salt solution to 200 cc. It is essential that this solution should be filtered, because it is liable to contain particles of glass that may have been chipped off from the glass beads during the shaking process.

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SYMPOSIUM ON SALVARSAN.

ARTICLE XXIV.

CLINICAL OBSERVATIONS ON THE USE OF SALVARSAN, WITH SPECIAL REFERENCE TO ITS ADMINISTRATION.

By HUGH CABOT, M.D.
of Boston.

READ JUNE 13, 1911.

Any judgment which we may now form as to the indications for and the value of this drug must be purely tentative, as it is yet entirely in the experimental stage. Our peace of mind has not been enhanced to say the least, by the extremely variable and rapidly changing theory of its effects emanating from its discoverer and his associates. We were at first asked to regard this as the therapia sterilans magna, and we were told that the function of this drug was to destroy the spirochaete at one fell swoop. It was pointed out that any dose smaller than the maximum was extremely likely to result in an acquired immunity on the part of the organism, and that the drug became less and less active. This theory must have commended itself to thoughtful students and we seemed upon the brink of having achieved a miracle, when lo and behold comes the information that the best effect of the drug is produced by multiple small doses. This entire change of heart has come through the failure of the drug to live up to the expectations raised by early work. We have had to admit that it is not a miracle, that one dose does not destroy all the organisms, and that in truth it behaves very much as do other drugs, and not wholly unlike our friend and ally, mercury.

Our judgment has been further confused by the conflicting evidence in regard to the best preparation. At the outset the acid solution was used, but this was promptly abandoned as being too toxic. Then we were urged to use the neutral solution, which was in fact not a solution at all, but a suspension. The drug is not soluble in an exactly neutral medium, and when this is obtained a precipitate forms which may be either yellowish or grayish. It is a good suspension to be sure, but one cannot but question the evenness of its absorption, whether or not it may become encysted and some of it be absorbed only after long periods, if at all. From this dilemma we emerged to the use of the alkaline solution; an irritating preparation to be sure, but nevertheless a real solution, upon the absorption of which one could rely. It is this preparation which the great majority of observers are now using, the effort being to obtain a solution in as mildly alkaline a medium as possible.

The decision as to the efficiency of the drug has also been confused, because many observers will insist upon regarding as a cure any retarding of the disease. Now it must at once be obvious that the question of cure will not be decided in one year or even in three years, and that a very considerable time must elapse before we can intelligently compare this last comer in the field with mercury and the iodides, with whose effects we have long been familiar. It must constantly be kept in mind that all we can expect for the present is a prompt and lasting effect upon present lesions, the question of cure being left wholly for the future to decide.

I will now review for you briefly our experience with the drug at the Massachusetts General Hospital, where it has been used chiefly under the auspices of the Skin Department.

METHOD OF ADMINISTRATION.

With one exception we have used entirely the alkaline

solution which has been prepared for us by Mr. Godsoe by the method described by him to-day.

INTRAMUSCULAR INJECTIONS.

The solution varies in quantity from 16 to 19 cc., the difference in amount depending entirely upon the variable solubility of the drug.

CHOICE OF SITE FOR INJECTION.

In all our cases we have employed the buttock, though the infrascapular region and lumbar muscles have been frequently used by other observers. We have selected the buttock because we believe that absorption is here reasonably prompt, that it produces rather less pain than in other regions, and should abscess formation occur less damage will be done. We prefer the upper outer quadrant of the buttock, care being taken to avoid the region of the sciatic nerve. The infrascapular region has seemed likely to be unnecessarily painful and one where considerable damage might be done by abcess formation. The lumbar muscles have been extensively used by many sound observers, and the work of Meltzer makes it appear that the absorption through this muscle is more prompt than elsewhere. In spite of this advantage we have thought it best to continue to use the buttock, as it seemed to us a very satisfactory location.

The skin is prepared by the ordinary method of scrubbing with soap and water, followed by the application of alcohol. The injection is made with a syringe holding 10 cc., with a needle not less than three inches long. We have found 10 cc. all glass syringe of Luer well suited to this work. As the amount of solution is in the neighborhood of 20 cc., 8 or 9 cc. are injected in each buttock.

IMMEDIATE RESULTS.

When using the alkaline solution the injection is always followed by pain. This is somewhat variable in amount,

often requiring morphia for its control during the first twenty-four hours, but not infrequently being satisfactorily mitigated by asperin. It is most severe during the first twenty-four hours, but persists as a dull ache, not infrequently referred along the sciatic nerve, for a week, sometimes ten days. The local results of the injection are interesting, somtimes acutely so. There is considerable infiltration and some edema. In a few cases, with us about ten per cent., there has been a blush of the erysipeloid type covering the buttock and extending upward into the lumbar region.

Some of the cases had every appearance of going on to abcess formation, but this occured in only one, an ill-nourished child of nine months. Other observers, however, have had abcess formation in from 5 to 10% of their cases. These sterile abcesses are excessivly chronic, heal slowly and occasionally require excision of the slough. Constitutional symptoms, fever, increased pulse rate, general malaise are observable, to some extent, in practically all of the cases. In only one or two of the cases did the temperature exceed 101, and in most it was below that point. Change of pulse rate was unimportant, and all constitutional disturbances subsided in two or three days.

EFFECT UPON THE KIDNEY.

It is known that arsenic in general and this salt in particular at times has an unfavorable effect upon the kidney. No untoward results have been noted in our cases beyond a moderate amount of diminution in the amount of urine, but we have not used the drug in any case known to have chronic dephritis.

As a result of the disturbances above noted it has been necessary to keep these patients in bed for a week or ten days, and some patients have been too uncomfortable to be about for two weeks.

INTRAVENOUS METHOD.

For intravenous injections we have used the alkaline solution diluted up to 200 cc. In this dilution it has a very mildly alkaline reaction. A great variety of different kinds of apparatus have been used for administration, and it may be given either by direct injection with a large syringe, or by the use of a reservoir which can be elevated to the desired height. The apparatus which we have used consists of a reservoir connected by rubber tubing with a two-way cock which is again connected with a suitable needle. At the two-way cock is connected a 10 cc. syringe filled with salt solution, which we have found useful in ascertaining that the needle has entered the vein and the blood is flowing freely, also in preventing clotting of blood in the needle and in washing the salvarsan away from the point of injection in the vein in order to diminish the probability of thrombosis.

The vein selected for injection has been one of the large veins in the neighborhood of the bend of the elbow. The skin is prepared as usual, and a tourniquet lightly applied to the upper arm so as to produce venous stasis. If the veins stand out prominently the needle may be plunged directly into the vein with very satisfactory results. In our experience, however, it has generally been easier to infiltrate the skin with cocaine and expose the vein for about half an inch. It can then be punctured by one not particularly skilled in intravenous medication, with ease and certainty. It is particularly important to make sure that the needle is in the vein and the blood flowing freely. In a few cases when the needle was believed to be in the vein the flow of blood was unsatisfactory, and injection of salt solution showed infiltration of the surrounding tissue. Should this infiltration be made with salvarsan, marked irritation will occur, and the injection be nearly, if not quite, as irritating as the intramuscular. We believe it therefore to be

important to test the satisfactory nature of the flow with salt solution before allowing the salvarsan to enter. When this precaution is observed the wounds heal quickly by first intention.

THE IMMEDIATE EFFECTS.

The intravenous injection when executed with good technique is painless. On the other hand it always causes some nausea, frequently a little vomiting and always some diarrhea. All these symptoms occur during the first twenty-four hours. In one case we have had a transient headache, but in none of the cases has the patient been confined to to bed more than twenty-four hours. If the technique is faulty considerable irritation about the wound may result in an amount of discomfort varying almost directly with the amount of salvarsan infiltration of the tissue. One of our cases showed thrombosis of the vein throughout its entire accessible course, and should this occur it must cause some uneasiness in regard to the possibility of embolus. We think it probable that improved technique will avoid the occurrence of thrombosis and its attendant possibilities.

OBSERVATION UPON THE EFFECT OF SALVARSAN ON THE SYMPTOMS.

Our observations are based upon a study of 50 cases, most of which have been selected because they were doing badly under the recognized methods of treatment, or were the subjects of some emergency condition. These emergencies may either be due to the presence of lesions endangering important organs, or may be of a social character. We have made exceptions to this rule in a few early cases and a few cases of tabes of syphilitic origin.

EARLY CASES.

This group includes seven early cases with primary lesions, some having also early secondary symptoms. They were treated with one intramuscular injection, and four have re-

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mained well, while three have had recurrences. Of the cases showing recurrences, two have been injected again, one is free from symptoms, and one has again relapsed. Of those free from recurrences, two have been taking mercury, one has been lost sight of, and one is apparently well three months after injection.

SECONDARY CASES.

There have been 28 cases showing secondary lesions. In these the immediate effects were in general excellent, most of them showing a prompt disappearance of the symptoms. In four cases a single intramuscular injection relieved, but did not cause disappearance of the symptoms. In the balance the lesions healed, but recurred in 12 cases. The effect upon some of the late secondary lesions was very striking.

TERTIARY CASES.

We have had six cases having so-called tertiary lesions. In these the benefit has been less striking. Three are apparently well, and three are known to have recurred.

CONGENITAL CASES.

Of the two cases belonging to this group, one, a child, was lost sight of at the end of a month, when it was apparently well. The other, a boy of twenty-one, is much improved at the end of three months.

CASES OF TABES.

We have injected five cases of tabes, one by the intramuscular and four by the intravenous method. In none has there been any apparent benefit.

Before venturing to express any opinion based upon our use of the drug, I would again remind you that our experience with the drug has been limited and extends over a short period of time. It must be purely tentative, and threfore subject to change.

CHOICE OF METHOD.

The intramuscular method is safe, painful, causing considerable confinement to bed, and consequent loss of time, and results in abscess formation in some cases, possibly as much as 5%. The drug is absorbed much more slowly than when given in the vein. It exerts its influence over a considerably longer period, and I am at present of the opinion that it produces a more profound effect upon the disease.

The intravenous method is technically more difficult, it is painless, causes but little confinement to bed with loss of time, but is not in the light of our present knowledge an entirely safe proceeding. Its effects are prompt, but I doubt if they are as lasting as that produced by the intramuscular method.

CLASSES OF CASES IN WHICH INJECTION MAY BE DANGEROUS.

Patients having disease of the optic nerve, organic lesions of the heart with failure of compensation, or marked disease of the kidney, are likely to react badly to this drug, and for the present at least it should be used cautiously and given only with a full knowledge of the possibilities.

I might sum up my view by saying that the intramuscular method is best suited for the use of the general practitioner, and that some knowledge of surgery, or at least of intravenous medication, will be required by those using the intravenous method.

Prediction is notably dangerous, but my present small experience leads me to suggest that some modification of the intramuscular method which will remove or mitigate the pain is likely to become the method of election. I still cling to the view that the production of a profound effect upon the disease is desirable, and that the use of repeated small intravenous injections is less likely to produce this

effect. In the light of our present knowlege I think we may safely affirm that the drug is a valuable addition to our means of treating syphilis, but it has yet to be shown that it will revolutionize our practice.

SYMPOSIUM ON SALVARSAN FROM THE CLINICAL STANDPOINT.

ARTICLE XXV.

PERSONAL IMPRESSIONS OF THE VALUE OF SALVARSAN.

By S. POLLITZER, M.D.
OF NEW YORK CITY.

READ JUNE 13, 1911.

So much has already been written on the subject of salvarsan that it would seem to me desirable now to wait with any further expressions of opinion till sufficient time has elapsed to enable us to form a final judgment on the subject. We know to-day probably as much of the action of salvarsan as we are likely to know for some years to come, and indeed it must be left to the next generation to assign a true value to the action of the drug. We know to-day after a year of trial of the drug little more than its immediate effects on the symptoms of the disease. It lies in the very nature of syphilis in all that 400 years of clinical observation has taught us that the value of any cure for this disease can be determined only after the lapse of at least a score of years. We cannot say with certainty of any case of syphilis that it is cured till the patient has died of old age without having manifested any symptoms of his disease for a long perod. This is of course an extreme view, and theoretical rather than practical. We know that many syphilitics recover entirely from the effects of their original infection; that they marry, produce healthy children and live to old age without showing any further symptoms of their disease. But we cannot with certainty say

of any patient that his disease has been eradicated. It is no uncommon occurrence to find gummatous lesions developing in a patient who has shown no signs of his disease for a period of twenty or thirty years or more. Even the Wassermann complement-deviation test is of little help in answering the question as to whether a given case is cured or not; the patient's blood-serum might give a negative reaction for many years and still later become positive. It complicates the matter that the late gummatous lesions occur in a fairly large proportion of cases that yield a negative reaction. It is evident therefore that we have no absolute criterion of the cure of syphilis. Practically, however, we may regard that man as cured who after several years of treatment shows no signs of his disease, who remains free from any such signs for a number of years, and whose blood-serum constantly yields a negative Wassermann test throughout a number of years.

From all this it follows that any discussion of the action of salvarsan in syphilis can have only a relative value. We can discuss only the immediate effects of the drug, and must be clear in our minds that we cannot possibly have more than a shadow of knowledge of its probable ultimate effect on the disease as a whole, as distinguished from its effect on the lesions of the disease.

I have been asked to present in this discussion my own experiences with salvarsan and my personal impressions of its value. In the first place, a few words as to the technique. You know that salvarsan is employed in dilute alkaline solution for intravenous injection, in concentrated alkaline solution for intramuscular injection, in suspensions in paraffine or oil, and in suspension of the neutral drug, its sodium salt, in water. The last method, the neutral suspension and the Wechselmann technique of interscapular injections was extensively employed in the early months of this year of salvarsan, and, to judge from a number of cases

that have come under my observation, is still the favorite method with the general practitioner. It is, however, the least effective of all the modes of application of the drug. Its subcutaneous injection is invariably followed by a tumor which persists for many months, which often becomes very painful after several weeks, which occasionally breaks down into a large and obstinate ulcer, and which frequently retains in local most of the salvarsan injected with the result that the remedy fails of any general effect. Much of the disappointment that has followed the use of salvarsan is attributable to this technique. It has generally been abandoned by those who have had any considerable experience with the drug.

The intramuscular injections of the alkaline solution, the method of Alt, is probably the most effective of the methods of employing the drug percutaneously. It has the disadvantage of being extremely painful, the patients often requiring an opiate the first day or two after the injection, and the dose, as with the neutral suspension, must be freshly prepared for each case.

The suspension of the unchanged drug in paraffine oil as first proposed by Kromayer, or in iodipin as I have been using it, has at least this advantage over all other methods, that the suspension is easily prepared, and that it may be made up in large quantities, so that it may be kept on hand in the office or in the clinic ready for use as occasion requires. The use of an iodopin-lanoline ointment containing 40% of salvarsan was proposed by Schindler, an assistant of Neisser, but the exact formula has never been published and such an ointment is difficult to handle and hard to inject. I prepare the iodopin suspension by simply triturating thoroughly in a warm sterile mortar the contents of ten or twenty ampules of salvarsan in sufficient warm iodopin to give a 40% suspension of the drug, and putting it up in a dark glass bottle. It keeps indefinitely; I have used

it without harm and with good effect as long as three months after its preparation. I make the injections with a glass syringe into the erector spinæ muscle as recommended by Meltzer, at about the level of the second or third lumbar vertebra. The injections are but slightly painful at the time, but considerable pain follows after two or three days if the patient puts any strain on the muscles of the back.

As to the value of this mode of exhibiting salvarsan, it is my opinion that it is more effective than the Wechselmann technique, and but little if at all inferior to the Alt method. Such estimates of relative effectiveness, however, are difficult to make; I can give only my impressions. I have seen a patient with secondary lesions in the throat that for weeks had made the swallowing of even liquids a torture, eat solid food three hours after an injection of the iodopin-salvarsan suspension; I have seen bullous lesions in an infant with hereditary syphilis dry up in twenty-four hours, and the rash disappear within a week after such an injection. One need therefore have no fear that in this convenient method of using salvarsan the drug may possibly not be absorbed.

With regard to all these methods of employing salvarsan, the subcutaneous and the various intramuscular injections, it must be noted that the presence of the drug, neutral, alkaline or acid, produces necrosis of the tissues with which it is in immediate contact. The wall of necrotic tissue must certainly to some extent prevent or at least delay the absorption of the drug, and it seems to me likely that the striking immediate effects which follow these injections result from the absorption of the moiety of the drug that is taken up before the wall of necrotic tissue is formed. If the necrotic abscess remains sterile no great harm is done the patient, and the tissue-detritus together probably with the remnant of salvarsan is gradually absorbed. This absorption may I think be favored by the exhibition of iodides. A patient with

very extensive gummatous lesions of the skin and the bones and a + + + + Wassermann reaction received four injections of salvarsan between December 3d, 1910, and Feb. 25th, 1911, three of them intramuscular and one intravenous. All his lesions were healed within a month of the first injection, but his Wassermann reaction as late as April 8th, four months after the first injection, and six weeks after the last, remained + + + +. He was then put on moderate doses of potassium iodide with the result that five weeks later, May 16th, his Wassermann reaction was negative. I should ascribe this action of the iodide, which we know does not *per se* influence the Wassermann reaction, in part to its effect in releasing unabsorbed masses of salvarsan and in part to its causing at the same time the softening of syphilitic infiltrations and thereby exposing encapsuled spirochaetae to the action of the freshly released salvarsan. This is of course only theoretical, but I have several times had this experience with iodides in cases of syphilis in which the lesions have healed under salvarsan, the Wassermann reaction remaining positive; a few weeks of iodides converted the positive into a negative reaction.

I need say but little about the intravenous infusions of salvarsan. The intravenous method is by all means the most agreeable from the patient's point of view. The little operation gives no pain beyond the prick of the needle and a systemic reaction (chill, temperature and vomiting), occurs in only about half the cases, and is a passing phenomenon of very little consequence. I have never seen any effect on the kidneys beyond a slight albuminuria with a few hyalin and granular casts coming on about twelve hours after the injection and disappearing by the next day. The drug, thus introduced directly into the circulation, necessarily exercises the full effect of the entire dose. It is, however, rapidly excreted, and it can therefore destroy only those spirochaetae which are at the moment so located that

the salvarsan comes in contact with them. If all the organisms of syphilis were in the blood as are the spirilla of relapsing fever in that disease, a single intravenous injection of salvarsan would probably cure syphilis as certainly as it cures relapsing fever; but in syphilis the organisms are in the tissues, and furthermore some of them are sure to be enclosed in dense fibrous tissues or in the midst of compact syphilomata, and these organisms probably escape the action of the drug in the short time that it remains in the circulation. It is a good plan therefore to follow the intravenous infusion in four or five days by an intramuscular injection to spread the action of the drug over a longer period; and this is the plan I have followed whenever circumstances have permitted.

As to the immediate effects of an injection of salvarsan on the active lesions of syphilis, it can scarcely be necessary to speak at this late day. Its effect may be summarized as far superior in promptness and efficiency to that of mercury and iodides. The healing of gummatous ulcers may be said to begin almost immediately after an injection of salvarsan, and the time required for complete epidermization of an ulcer depends only on its size. I have dealt for many years with a fairly large number of cases of syphilis, which I have generally treated with very large doses of mercury by intramuscular injection. My normal dose of salicylate of mercury is 2½ grs. (0.15) and bad cases get as much as 5 grs. (0.3) at a dose. I speak therefore from a considerable experience with syphilis and with intensive treatment with mercury, and I do not hesitate to say that the curative action of salvarsan on the active lesions of the disease, as a rule, far exceeds that of mercury. At the present time I have not a single case of syphilis that has been under my care for more than three weeks that exhibits any lesions of the disease. At no time in the past twenty years, before the introduction of salvarsan, could I have made such a statement.

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As to the permanent value of these symptomatic cures, after a single injection of salvarsan, the matter is quite different. In the first months of our experience with salvarsan, under the influence of the brilliant idea of a *sterilisatio magna*, we were trusting to a single injection of the drug. There were some complete failures, especially with the Wechselmann technique, and many recurrences after only a few weeks. But since I have employed intravenous and intramuscular injections, and especially since I repeat the injections at intervals of four, six or eight weeks, till the Wassermann reaction becomes negative, I have not seen any lesions of syphilis develop even after six months, and this applies to all kinds of cases in every stage of the disease. With repeated doses of salvarsan I have thus far kept my patients free from any visible lesions of their disease.

As to the ultimate effect of this course of treatment, time only can tell. The effect on the Wassermann reaction, however, affords us perhaps some index of its value. In the literature, the reports on the Wassermann reaction after salvarsan are at present of very little service. They deal promiscuously with cases of early and of late syphilis, and they include cases treated by various methods and with various doses. To bring about a negative reaction in the early stages of the disease is not a difficult matter. Three months of mercury will generally do it, a couple of injections of salvarsan will quite certainly do it. It is not so with the later manifestations of the disease, with the cutaneous and especially the bone lesions of the tertiary period. Taking all the cases together, a negative Wassermann reaction seems to result from a single injection of salvarsan in about 20% of the cases, in from four to eight weeks after the injection. This corresponds with my own experience. The early cases considered by themselves will give a much higher percentage of negative reactions, the late cases a

lower rate. But with repeated injections of salvarsan the results in my experience are quite different. I have to-day very few cases of syphilis treated with salvarsan that still show a positive Wassermann reaction; between 80% and 90% of the cases treated more than eight weeks ago are negative. In a few obstinate cases this result was brought about by the exhibition of potassium iodide after salvarsan as I have mentioned above. In another class of case, refractory tertiary lesions treated for a long time with large doses of mercury and still giving a positive Wassermann reaction, a single dose of salvarsan promptly healed the lesions and converted the positive into a negative reaction. I may say here that I regard a positive Wassermann persisting after a long course of mercury as a clear indication for salvarsan.

A word as to the dangers from the use of salvarsan. In my own experience I may say at once it has been my good fortune to have escaped with only a single serious accident from the use of the drug. This is, no doubt, purely a matter of chance, for serious accidents have happened to some of the most careful observers. I have, however, been very conservative in the use of the new drug; I have given it only when I considered it clearly indicated and, of course, in cases that showed no grave cardiac or renal disease. I am still treating the majority of my patients with mercury. The single serious accident to which I refer is a case of deafness in one ear that developed after a second injection of salvarsan. The patient was a man in the secondary stage of his disease who had received an injection of salvarsan from Prof. Wechselmann in Berlin last August. On the recurrence of lesions in December, I gave him an injection of salvarsan in iodipin. Within a week his lesions had disappeared, but he noticed a total deafness in one ear, which which was diagnosed as due to a lesion of the vestibular branch of the auditory nerve. The patient has since moved

to the West, and I heard last month from his physician that function in the affected ear was almost restored. A number of such cases have been recorded, and Ehrlich explains them as due to the hyperæmic swelling of a syphilitic focus in the nerve, a kind of Herxheimer reaction in the narrow bony channel in which the nerve runs. The prognosis in these cases is generally good; the deafness is not permanent.

As to injury to the optic nerve analagous to the optic atrophy that has occurred after other arsenical drugs like atoxyl and arsacetine, there seems to be little, if any, danger. Optic neuritis occurs in a not insignificant proportion of cases of syphilis even in an early stage of the disease, and should not of course be ascribed to the salvarsan. Since the introduction of this drug our cases of syphilis have been studied and watched as never before. Wechselmann's statistics of cases examined before treatment with salvarsan show that more or less grave lesions of the optic nerve without loss of vision occur in about 3% of cases of syphilis in an early stage. Becker's statistics, made two years before the introduction of salvarsan, give about the same proportion of optic nerve disease in early syphilis. If salvarsan affected the optic nerve it is reasonable to suppose that it would produce a lesion like that of other arsenical poisons; a descending peripheral atrophy of the nerve. The cases of diffuse optic neuritis that have been recorded as developing after salvarsan, may safely be ascribed, I think, to the syphilis, and not to the salvarsan. I have recently seen my first case of this kind.

A young woman with a recent infection, a generalized macular rash, a sore throat and a 4+ Wassermann reaction received 0.4 salvarsan in iodopin April 29th. I did not see her again till June 10th. Her throat and cutaneous symptoms had disappeared within ten days, she had gained in weight and was feeling well. She complained, however, that there had been a blurring of vision in the right eye for

the past three days, and an examination disclosed a beginning optic neuritis of that eye, with a slight congestion on the other side. I gave her at once an injection of the salycilate of mercury, and the next day a second injection of salvarsan in iodipin.* This treatment illustrates at least my personal conviction that the optic nerve lesion is a part of the syphilis and not an effect of the salvarsan. I should, however, decline to give the drug in any case of syphilis which had previously been treated with an arsenical preparation.

A number of deaths have occurred after salvarsan, many more I have reason to believe than are recorded in the literature. These deaths, when they are not directly due to a grave error in technique, have occurred for the most part in feeble, debilitated, marantic individuals who would probably soon have succumbed to their syphilis, and in patients with diseased kidneys or heart-muscles. I have elsewhere referred to chronic alcoholism as a contraindication to the use of salvarsan, and I desire here to emphasize this point. These patients often have bad hearts and almost invariably grave lesions of the kidneys. In general I should say that there is no serious danger from the use of salvarsan in any ordinary case of syphilis with such fairly normal organs as we are likely to find in these cases, who for the most part are young people in the prime of life.

I should sum up my impressions of salvarsan in the treatment of syphilis by saying:

That it is the most effective drug we possess for the symptomatic cure of the active lesions of the disease;

That its best effect is produced by an intravenous followed by an intramuscular injection;

That while one or two injections are not likely to cure the disease they will materially advance its cure;

* The optic neuritis disappeared within three weeks; the patient received only a single dose of mercury.

That its use, together with treatment by mercury or sometimes by iodides, will bring about a negative Wassermann reaction far more certainly and more quickly than is possible with mercury alone;

That properly used it is practically harmless even in repeated doses.

SYMPOSIUM ON SALVARSAN FROM THE CLINICAL STANDPOINT.

ARTICLE XXVI.

PERSONAL EXPERIENCE WITH SALVARSAN.

By ABNER POST, M.D.
OF BOSTON.

READ JUNE 13, 1911.

In the excitement which has followed the introduction of Ehrlich's great remedy, we are in danger of undervaluing the drugs that we possessed before Salvarsan existed.

In 1895, Dr. R. W. Taylor wrote in his most valuable textbook: "The experience of more than three hundred years has shown in no uncertain manner that mercury has the most marked and salutary effect in the treatment and cure of syphilis, and that if properly handled it may almost be termed an antidote or specific for that dread and potent disease."

Another author wrote about the same time something to the effect that "In nothing does man approach so nearly the power of the Creator as in the use of the iodide of potash."

Before Salvarsan it was a relief to find certain lesions were syphilitic and not cancerous, for that made us sure that we could cure them. Persistent headache and osteoscopic pains vanished almost like magic before our old-time syphilitic medication.

Experiments on apes showed that it was possible to inoculate and then destroy the poison *in situ* by calomel ointment — and one daring medical student repeated the experiment in his own person with success.

There is no doubt whatever that in mercury and iodide of potash we have remedies that produce, at times, effects which deserve to be called miraculous; but the name of specific has come to be applied with less assurance. As our knowledge of the sequelæ of syphilis has increased mercury has grown less satisfactory. In the sentence just quoted from Dr. Taylor, it was said that when properly handled mercury was almost a specific, but there was no uniformity of opinion as to form of drug or method of administration. Under the older treatment, a series of years was required for a cure, but the great fault in medication has been that after no matter how many years there was no positive assurance that our patients were cured. No matter how faithfully, nor how long, nor by what method our patients were treated, some small percentage of them were sure to show late lesions of a more or less serious character. It has become painfully evident that mercury is a most valuable remedy, but that with our present methods it cannot be called an infallible cure.

Personally, I have been inclined to believe that in mercury we had the necessary agent, but that we did not know how to use it; that better knowledge and better methods of controlling dosage and gauging effects would eventually make it nearer the specific that it was once supposed to be.

Perhaps the final explanation of the difficulties in treatment will be that there are different varieties of spirochætæ, some of which are followed by late lesions, while others are not.

Syphilis seems to be a malady in which the natural history of disease has been neglected—or rather the supposed remedy has been studied in preference to the disease. Starting with the assumption that mercury must cure when properly handled, it follows, when the disease persists, that the remedy has not been properly handled—perhaps the doctor did not know;—perhaps the patient was unfaithful. What-

ever the explanation, it certainly is the fact that recurrences are too frequent. Serious late lesions occur probably in a small percentage of the total number of syphilitics, but they are much too common to allow us to rest content.

There have been for many years attempts to find more effective methods of treatment. The different combinations of mercury and iodine introduced within the last few years are very numerous, and Salvarsan, while its composition is based upon scientific data which differentiate it from its predecessors, is only the last of a series of arsenical preparations. In attempting to appreciate the value of a new remedy, it is necessary to recall that syphilis is a chronic disease. There is a pessimistic view held by some that it is incurable, though the more hopeful among us believe that it is incurable only in a minority of cases. It is characterized by remissions, during which it is utterly unrecognizable, and it is a disease which varies so much in symptoms as to present cases so widely different that it is hard to believe that they have a common origin. We must also remember that we have already remedies which, with all their faults, hold the very highest rank among remedial agents. There are to be considered, then, certain difficulties in estimating the value of new remedies.

In the first place, in speaking of Salvarsan, a single year is not a long enough time in which to fully test its value, but it is long enough to gain a very great deal of information.

It is already demonstrated that Salvarsan does not always cure the disease at a single dose. Recurrences are much too frequent to allow us to believe that they are accidental or exceptional. It is however recognized, that the signs of the disease disappear in a wonderful way after its administration.

The fact has been demonstrated that doses may be repeated without harm. The theoretical danger that a race

of spirochætæ would be raised, by the survival of those that outlived the first dose, that would be practically unconquerable, has fortunately not been realized. Second and third doses are perfectly practicable.

Syphilis presents so large a variety in its manifestations that we may properly consider the effects of drugs upon these separate varieties.

Having assured oneself that salvarsan is not going to be an absolue antidote, the first question may well be, "What will it accomplish in those cases which prove refractory to earlier methods of treatment?"

My cases are all from private practice, and are already so scattered that no attempt has been made to tabulate them. With my colleague Dr. Morton Smith, over 150 cases have been submitted to the action of the drug.

The first cases were carefully chosen from those which were not acting well under treatment by mercury and potassic iodide. They comprise a variety of lesions—destruction of the nasal bones, perforation of the skull, obstinate ulceration of the throat, recurrent iritis, keratitis, disease of the ribs, and of the testis—all of these cases improved with salvarsan.

My first case will illustrate this series.

His disease had lasted eleven years. It had disappeared quite readily at first and the patient had thought himself cured, but at the end of two years it had returned and attacked the bones of the nose, and for nine years in spite of faithful and intelligent treatment it had persisted. At the time I first saw him the nose was swollen and tender. There was slight discharge and odor, with headache. The bony structure was fast being eaten away, and I was warned by Dr. R. A. Coffin, who saw him for me, that if his nose was to be saved there was no time to be lost. For nine years he had treated it in different parts of the country, always putting himself under the best medical care available,

once spending several weeks at the Arkansas Hot Springs, where he had daily inunctions. After nine years of continuous medication, he could see only constant progress in his disease. The attitude of the profession towards the disease and mercurial medication was illustrated by a little incident in this case. Shortly before I saw the patient he called upon a throat and nose specialist in one of our large cities, who exclaimed on seeing him—"Why! how on earth did you *allow yourself* to get into such a condition?"—a most discouraging remark to a young man who had for nine years spent his best endeavors in attempting to better his condition.

This young man gladly submitted himself to salvarsan. I explained to him that I had never administered it, that it was my first case, and he answered that he would put himself into my hands, that I should do whatever I liked; for unless I could help him, he was hopeless. The drug was injected beneath his shoulder blade in neutral solution. He laughed at me when I explained to him that he would probably have pain. He had little rise of temperature. He was unwilling to stay in bed, because it was unnecessary. The week in the hospital that I had predicted for him he begged me to shorten. There was absolutely no necessity for so long a detention. At the end of a few days the fetid odor left his nose, the ulceration healed, leaving an absolutely healthy nostril behind it, in which no sign of disease could be discovered. Headache disappeared; appetite and sense of well-being returned, and the man was apparently well. He made long journeys to see me at intervals of a fortnight, and for three months he was apparently perfectly well. At the end of that time there was a little puffiness beneath one eye, and Dr. Coffin recognized a return of the disease.

His first subcutaneous injection was made in October. In February an intravenous injection was made. His

symptoms again disappeared, and when I saw him a fortnight ago there had been no return, but the lump beneath his scapula, where the first injection had been made, still remained and was still tender.

Surely here was a case which was most encouraging. What evidence could make one more enthusiastic over the new drug?

A series of cases followed very much of the same character, involving disease of bone, ulceration of the throat, disease of the testis, ulcerations, etc., all of which healed in the same wonderful manner. Various cases of obstinate desquamation and deep ulcerations of the hands healed. Of these, several cases have required a second injection, but all are manifestly better up to the present time from the use of salvarsan. Of these cases one has had an abscess underneath the scapula. I doubt very much if that was the fault of salvarsan *per se*. I do not believe that the injection was in his case properly prepared. Anyway, injected in October that young man has still an open sore beneath the scapula, which discharges but slightly. The slough, which was a chemical slough, and contained no sign of bacteria, has largely come away, but the wound is not yet entirely healed. This young man, in addition, had a recurrence of the deep ulcerations of the palm and has received a second application of the drug, this time an intravenous injection; all symptoms have again disappeared, and he is apparently well, save for the as-yet unhealed ulcer beneath his scapula.

With such experience in these mercury-resistant cases, one must of necessity go on to other cases. These cases were injected subcutaneously in the first three, eight intramuscularly, and since then I have used only the intravenous injection, and that directly into the vein by the Schreiber needle, without finding so far the necessity of cutting down on the vein. There are some 150 such cases, of which I

cannot give an actual tabulation. They are all cases of private practice. Some of them have disappeared. They are difficult to follow, but they have left upon my mind very vivid impressions.

The cases of the application of salvarsan to primary syphilis have not been numerous, some six or eight perhaps only, but the primary sore has healed without exception very rapidly, much more rapidly than is usual with mercurial applications. The danger to the community from these cases has been greatly lessened. A young girl, a domestic, inoculated with a sore on the lip, is an extremely good example. She was obliged to earn her own living. She lost her place as a domestic, very naturally. The sore, on the application of salvarsan, healed within a very few days and left her absolutely without any sign of the disease whatever, and no further signs have followed, though the time is long past when secondary symptoms ought to have occurred. She has found employment in a family who know the conditions. There has been accomplished in her case in a few weeks a result which would have acquired many months with the old method.

But while the primary sore itself heals with great rapidity, the glands in the immediate vicinity do not disappear so rapidly, and it has seemed wise in most of these cases to keep the patient upon mercury in addition to the original dose of salvarsan, and to hold oneself in readiness to repeat the salvarsan.

In secondary syphilis salvarsan is certainly of very great power, but the marked papular eruptions do not disappear as readily as one would hope; they fade somewhat, but absorb much more slowly than one would expect from the rapid action on the primary sore. The moist lesions—lesions of the lips, which make cases particularly dangerous to their associates—heal, almost without exception, with wonderful rapidity.

Of cases that may be called tertiary, I have practically already spoken in speaking of cases of mercury-resistant disease. Many of these cases have required the administration of the drug for a second time, and I cannot help feeling hopeful that they are on the road to permanent recovery. By no means, however, can one feel as yet absolutely sure.

Nervous cases are the ones in which one would like to see the drug act with good effect, but one cannot expect salvarsan or any other miraculous drug to restore nerves that are destroyed, but perhaps the drug acts in limiting further destruction. For instance, I have two cases of tabes in which the pain has been extremely great. One individual has certainly come very near dying at various occasions from the severity of the pain through the abdomen and down the legs. He has become a confirmed morphine taker, and is laid up every once in a while by severe crises. Salvarsan worked wonders in his case. The pains have practically disappeared. The amount of morphine that he takes has been diminished by at least two thirds. I am perfectly well aware that such crises are absent for a long time, and that it requires a very long time before one can feel certain that it has actually done such patients any permanent good; but, so far, that man and his family both affirm that the pains have been absent for a longer time than before, and he certainly has a great temporary comfort.

The same is practically true of tbe second case. The patient is not restored to health, but the uncertainty in his gait and his pains have diminished very decidedly.

There is one very interesting case which has come to my notice in which the value of the drug is as yet undetermined because the test has not yet been fully carried out. This was the advent of a man and his wife, in my office, who told a story something like this: They have a perfectly healthy child six years of age. At about the time this child was

born the man acquired syphilis. In the intervening six years the mother has given birth to two children, both of whom died with syphilis a very few days after birth. The mother has shown absolutely no sign of syphilis, though she has borne and nursed these two syphilitic children. Now this father and mother desire healthy children. The Wassermann reaction in both father and mother was positive, and the positive Wassermann reaction in women of that class is extremely interesting. We know that women who have borne syphilitic children are practically syphilitic, although without signs of the disease. The Wassermann reaction has been found to be positive not only in this case, but in a long series of cases in the hands of other observers. This husband and wife have each of them received salvarsan, and the result in their family can only be determined at some future date.

The dangers of salvarsan most certainly cannot be passed over. Deaths, deafness and blindness have all been reported as results of its administration, none of which have so far happened in my experience; but even a very limited experience with the drug affords evidence that it is a drug of dangerous possibilities. When it is given intramuscularly, it is attended by very great pain, and the patients almost without exception require morphine during the first twenty-four hours. When it is given intravenously, there is no pain unless some portion of the drug is injected into the tissues outside the vein, which fortunately can usually be avoided; but some form of intestinal disturbance is present in a very large proportion of cases. Some patients are nauseated, some vomit and vomit seriously, some have serious diarrhea sometimes lasting for two or three days, though usually only an hour or two, and it has been my fortune to meet with one case of scarlatinaform eruption. This occurred in an individual with cerebral disease. After his second injection he had a very high temperatnre, and in the

morning was covered with a scarlatinaform eruption which was very bright, which, however, disappeared within forty-eight hours.

Two of my earlier cases of intramuscular injection had an experience which caused temporary alarm. In one the elbow, in the other the ankle, within forty-eight hours of the injection, became swollen, hot and painful. There were no unusual symptoms at the site of injection. The afflicted ankle joint became normal within a few days. The man with the elbow did not return to report at he should have done, but was known to be using his arm a few weeks later in his ordinary occupation in a perfectly normal manner.

While the experiences with the drug at my hands have never been dangerous, they have been so marked as to make me feel that we are dealing with a dangerous drug; that every dose is an experiment; that sooner or later we shall meet with some individual who is especially sensitive to arsenic, with perhaps fatal results, and that fatal results cannot be absolutely avoided except by sticking to a dose which experience has shown to be practically safe.

I cannot help feeling that there is a certain analogy between mercury and salvarsan. Both of them are toxic; with both we get apparent cures; with both we get recurrences. Salvarsan certainly acts more quickly. It certainly gives us apparent cures in cases in which mercury has failed; but we find occasionally cases in which the real value of salvarsan is doubtful. Most of these cases, I am glad to say, occurred during the earlier demonstrations of the drug, so that we may hope that they were due to faulty technique or dosage. We do not yet know that our patients are cured, but we certainly have in this drug a wonderful addition to our therapeutic resources, not necessarily an antidote, but certainly a wonderful help. I cannot help feeling that it makes possible the public care of the disease as was never before possible. Boards of health have hesitated

to take charge of cases which they must keep for months before they could be considered safe to the public. The possibility of removing the contagious symptoms within a few hours or days puts a different aspect upon the matter and renders the care of such patients practicable.

It is not to be expected that this remedy will remain as it is. Ehrlich or some one will doubtless modify it in such a way as to make it more stable and easier of application. Perhaps it will be entirely superseded by another remedy even more reliable. Without doubt it is the first step towards a future of which one can only dream.

Certainly, I cannot but believe that the control of syphilis is much nearer at hand than it has ever been before.

There is one further effect of salvarsan that is worthy of mention. Syphilis has been a tabooed disease, which one spoke of only with reticence and with careful choice of the audience. Salvarsan has introduced it to the community. It is now possible to mention it in newspaper items or magazine articles. I am sure that such publicity will go a long way towards helping us to recognize it, to study it, to treat it and eventually to cure it.

DISCUSSION OF THE SEVEN PAPERS ON SALVARSAN.

DR. BOOS: I would like to say that I think that the method which Dr. Pollitzer uses to dissolve the "606" in the very same receptacle in which it is mixed has an element of danger. In a recent medical paper a man has published a method for the intravenous application of "606" in which he uses a very complicated apparatus. It will do away with the possibility of any sediment of any substance which might possibly do harm in the vein if carried into the vein. Very frequently little splinters of glass which are not observed when you open the salvarsan, get in. I think it only fair to the patient, and wise in every case, to do something to prevent such sediment from being carried in. Sometimes it is a thread, sometimes one of those little fine particles of the gelatinous particles of salvarsan which has not gone into solution. Even by Dr. Pollitzer's method that might happen; so I think it wise always to filter the solution through a sterilized filter before putting it into the veins of the patient.

DR. E. MYERS: I would like to ask the amount of salvarsan used; that is, has there been a change in the amount decided on at the meeting on Salvarsan at the Massachusetts General Hospital, when, I believe, it was four-tenths of a gram for one-hundred pounds of adult weight?

DR. BOOS: I can give Ehrlich's statement published last month. He said that six-tenths of a gram intravenously for an adult was too large a dose, and recommended four-tenths of a gram for an adult, irrespective of weight, to be repeated twice intravenously.

DR. TOWLE: I think the gentleman refers to a statement I made. I said in the course of our investigations we had not been able to decide upon any definite dose. We accepted these statements of Ehrlich, that it should be so much and so much, but so far as any rule was concerned we had been absolutely unable to discover any. It occurred

to me that it might be of value, therefore, to determine so nearly as possible the proportion of the weight of the body to the size of the dose, and to try in that way to determine definitely the relationship, then to follow the cases through and to compare the results with the different proportionate doses, hoping that we might might get some line upon a definite rule for dosing. That was nothing definite—simply a suggestion made at the Massachusetts General Hostal meeting.

In response to a question whether in giving salvarsan to the mother of a nursing child, that mother should be allowed to nurse her child, and if the arsenic will affect the baby or whether it will not,

DR. BOOS said: There is an article which was published only last week that goes into detail on that question. The two original men who spoke of the mother nursing her child found, after the intramuscular injection of salvarsan, the child was benefited. The syphilitic symptoms disappeared. After the intravenous injection this was not the case. In the cases described in detail the children, instead of improving, became very much worse. A peculiar thing happened about 36 hours after the nursing; the children, in both cases, had a very typical syphilid—a true syphilitic picture. These children became steadily worse. He found, as Dr. Pollitzer said, that the only thing that bettered them was to take them from the breast and administer potassium iodide and ordinary cow's milk.

DR. POLLITZER: I want to say just a word in regard to the use of the salvarsan unfiltered. Dr. Boos spoke of the danger of the splinters of glass. That has been referred to. Well, of course, I use no glass beads. There is no necessity for it. The salvarsan is put directly in a large quantity of water. The water is taken first and the salvarsan dropped carefully on the surface of the water and then agitated. There is, of course, nothing added which should give a precipitate.

If one uses clean water and nothing but salvarsan and a clean vessel one has a clean solution. There are no gummy particles of salvarsan if the water used is hot and one agi-

tates sufficiently long. In other words, I get a perfectly clear solution.

As to the danger of splinters of glass, that of course exists, but in the first place, inasmuch as the dose of salvarsan contained in the ampules is at least six decigrams and one is not going to use at most more than five, perhaps not more than four, I have made it a practice after opening the tube of salvarsan, which I hold at a slightly oblique angle, to gently tap it so a little of the salvarsan spills out. That will take out any particle of glass which may have fallen down into the tube.

They have spoken of 200 c.c. of water. I think that it is a mistake to make it so concentrated. It is evident that this solution is an irritant to the tissues. If it were not for the time consumed and for other reasons we would do better, I think, to make our solution in 600 c.c. of water, not 200 or 300. I have now a solution of 300 c.c., and I am not going to use it all. I therefore hold my burette vertically for a few minutes or let the nurse hold it while I look after the preparation of the arm of the patient. In that time any little particles would have settled, particularly glass; before making the connection with the rubber tube or the needle, I open my stop-cock and allow anything that is settled there to flow off, allowing perhaps 50 c.c. of the entire fluid to run out. That will take out any possible particles in the solution.

Dr. Boos: I have just one more little objection to Dr. Pollitzer's method. In his apparatus he had no method to clear the needle. There are certain cases of syphilis in which the blood coagulates very rapidly, and where after introducing the needle, when you connect to make the injection the infusion simply won't go through the needle. To have some apparatus connected with the infusion apparatus is a very good idea, and I think it is a wise precaution to have one. Perhaps Dr. Pollitzer never had a case with such great tendencies to coagulation.

ARTICLE XXVII.

CIRCULATORY DISEASE.

ITS PREVALENCE IN NEW ENGLAND, MASSACHUSETTS AND BOSTON.

By EDWIN WELLES DWIGHT, M.D.
OF BOSTON.

READ JUNE 13, 1911.

Statistics from many sources demonstrate that the work of the medical profession during the past twenty years has been effective; that in the saving of life, the increasing of efficiency, and the diminishing of suffering, much has been accomplished.

Statistics are cold and dreary facts, of little value if they be not wisely interpreted; and they are dangerous in the hands of the special pleader. But statistics have their value, and a great value, if the volume is large enough to be handled to advantage, and properly interpreted.

It is true that statistics show a great improvement in mortality as a whole; that the average duration of life has largely increased, and that the chances of a new-born infant reaching the age of thirty have been multiplied. But this increase in the expectation of life has taken place among the younger ages, and, while the number reaching the age of thirty has increased, the proportion of those of that age who live to be seventy has not only relatively, but actually, diminished.

The very fact that, as a result of modern treatment and sanitation, the lives of a large number of infants have been

saved, carries with it, as a natural sequence, a large number of individuals reaching young adult life who, as a result of low vitality and impaired resistance, would, under former conditions, have died.

The lives of many such children are now saved, but it should not be forgotten that as they increase in years they carry with them some of that impaired vitality which they inherited or acquired in early life.

Statistics are available from various sources. The best and most reliable are found in Census reports and the records of life insurance companies.

The following tables from the Census of 1908 show the changes that have taken place since 1900 in the relative frequency of the more common diseases, tabulated as "Causes of Death," to 100,000 of the population.

It will be noticed that all which I have classified as "Preventable and Infectious" show a material diminution, except Scarlet Fever. That among those broadly classified as "Circulatory," all show an increase except Pericarditis, which would probably be placed more accurately in the "Infectious" group. That in the "All Other Diseases" group the increase and decrease is about equal, but on the whole a diminution.

I.

PREVENTABLE AND INFECTIOUS DISEASES.

1900–1908.

Pneumonia (all forms)	–44.5
Tuberculosis (all forms)	–27.3
Diphtheria and Croup	–21.0
Typhoid Fever	–10.6
Diarrhœa and Enteritis (under 2 years)	– 9.9
Diarrhœa and Enteritis (2 years and over)	– 7.3
Dysentery	– 4.8
Puerperal Septicemia	– 4.2
Measles	– 2.3

Scarlet Fever	+ 2.2
Tetanus	- 1.8
Smallpox	- 1 7
Whooping Cough	- 1.1

CIRCULATORY DISEASE.

1900–1908.

Heart Disease	+22.1
Diseases of Arteries	+11.5
Bright's Disease	+ 5.6
Apoplexy	+ 4.6
Acute Nephritis	+ 2.8
Endocarditis	+ 1.4
Pericarditis	- 1.3
Angina Pectoris	+ 0.3
Embolism and Thrombosis	+ 0.2

ALL OTHER DISEASES.

1900–1908.

"Convulsions" (under 5 years)	-16.9
Cancer	+11.3
Acute Bronchitis	-10.7
Paralysis	- 9.1
Peritonitis	- 8.5
Chronic Bronchitis	- 8.1
Diabetes	+ 4.2
Other diseases of the stomach	- 3.4
Appendicitis	+ 2.0
Cirrhosis of Liver	+ 2.0
Rheumatism	- 1.6
Biliary Calculi	+ 1.3
General Paralysis of Insane	- 1.1
Ulcer of Stomach	+ 0.8
Obstruction of Intestines	+ 0.7
Pleurisy	- 0.6
Hernia	+ 0.3

Census Table No. II. shows that in the whole country there has been an increase in the deaths from circulatory

diseases between 1904 and 1908 of 7 in 100,000 population. That the cities, as a class, have increased, while the rural districts show a diminution.

II.

	1904	1908
Average	172	179
Cities	168	181
States	181	183
Rural	182	175

Census Table No. III. includes all of those cities of over 100,000 population in which the mortality from circulatory disease approaches that of Boston, arranged in the order of the prevalence of such diseases in 1908.

It will be noticed that San Francisco is the only city of such a population which has a larger death rate from circulatory disease, and from San Francisco we have no statistics since 1905. Not only does Boston lead in this class, but the increase of such deaths during the past five years has been much greater here than in other cities, with the exception of Washington.

III.

CITIES.

	1904		1908
San Francisco, Cal.	296.7	(1905)	306.3
Boston, Mass.	229.1		257.9
Washington, D. C.	222.4		254.9
Philadelphia, Pa.	205.		208.
Baltimore, Md.	182.8		190.1
New York, N. Y.	178.5		177.
Chicago, Ill.	127.		140.7

Census Table No. IV. shows that of all the registration states the only ones where the increase in deaths from circulatory disease exceeded those in Massachusetts were Indiana, Vermont, and New Hampshire. New York is shown as following these four states in the relative increase.

IV.

STATES.

	1904	1908	Percentage of Increase.
New Hampshire			
Cities	171.1	213.9	25.01
Rural	244.0	243.7	.12 decrease
Vermont			
Cities	178.7	220.9	23.61
Rural	202.6	254.2	25.47
Indiana			
Cities	138.2	162.7	17.73
Rural	114.4	150.6	31.64
Massachusetts			
Cities	200.9	224.7	11.85
Rural	245.4	278.3	13.41
New York			
Cities	182.6	197.	7.89
Rural	225.6	242.9	7.67

Unfortunately, the statistics of life insurance companies have not been easy to obtain in the past, but I have every reason to believe that in the future conditions will change and that you will find at your hand a large amount of material which will be of service, not only in settling doubtful questions, but in bringing up new and practical ones for you to consider.

The statistics of life insurance companies have some advantages over those available from other sources.

First. Insurance companies deal in thousands rather than units, and in their combined investigations they are able to consider groups of lives running in number into the millions.

Second. On account of these numbers and the multiplicity of facts which are at hand it is possible for them to make their classes homogeneous.

Third. The statements upon which their figures are based being part of a contract, or facts upon which legal decisions depend, are, as a rule, accurate.

Fourth. Statistics from such sources are collected, tabulated, and given out by, or under the guidance of, the leading mathematicians of the country, and for that reason are not apt to be inaccurate or influenced by prejudice.

In considering statistics from any source it is necessary to understand their origin. A study of life insurance figures would be of little value if we failed to appreciate that the individuals considered are picked from above the average in the community. They are men and women who are saving money and protecting their families. They are of good habits, and they are in essentially good health when they come under observation. Having these facts in mind it becomes safe to draw conclusions from the figures which are presented.

Insurance statistics, having a bearing on the prevalence of disease, are obtained from three sources:—

First. Statements of applicants as to their personal and family history. Open to some criticism on account of a possible lack of accuracy, but they are of value when taken in such large groups that the element of doubt is removed.

Second. Conditions found on examination. Essentially accurate, as the examinations are made by carefully selected physicians whose findings are checked in a variety of ways.

Third. Causes of death. May be open to criticism, but when carefully arranged and studied in large numbers can be roughly accepted as accurate.

The insurance company with which I am connected, and from the experience of which these figures are drawn, was chartered in 1835, and has been in active business since 1843. Its business has been obtained from all sections of the country, so that in the more complex groups are included

individuals who have been insured during the last sixty years from all sections of the country.

From all of the deaths which have occurred during that time a table has been prepared giving us a standard from which to work, and on the chart presented herewith will be found the general sub-divisions which are used in classification, showing the percentage of deaths from such causes as have been responsible for at least one-half of 1 per cent. of the total number. By reference to this chart it will appear that five conditions have been responsible for 54 per cent. of our deaths.

I.

Heart Disease	13.25%
Arterio-Sclerosis	11.25%
Pulmonary Tuberculosis	10.33%
Pneumonia	10.00%
Nephritis	9.25%

Of these, arterio-sclerosis, heart disease and nephritis can be clearly classified as "circulatory." If it is fair to assume that at least one-half of all of the pneumonias really died from the failure of a circulatory apparatus already impaired, we show a death rate of nearly 40% from circulatory disease.

Life Insurance Table No. II. shows the deaths from circulatory disease occurring in the years from 1844 to 1910, divided into "10-year" groups. It is to be expected that during the first thirty years of the business the percentage of deaths from circulatory disease would be small, for the reasons that the time which had elapsed after examination was short and that those persons entering the company at the average age of 35 had not had time to reach that period when circulatory disease is most common.

For these reasons the groups after 1874 are of most value as a comparison.

II.

DEATHS FROM CIRCULATORY DISEASE

BETWEEN

1844 and 1854	10.50%
1854 and 1864	15. %
1864 and 1874	25. %
1874 and 1884	34. %
1884 and 1894	40.50%
1894 and 1904	43.75%
1904 and 1910	45.75%

Life Insurance Table No. III. shows the percentage of deaths from circulatory disease divided by groups of States. During the first ten years of this company's experience, from 1844 to 1854, its business was written almost exclusively in New England, and the same is true, to a less extent, of the following ten years; therefore, the percentages of deaths during those two periods may be compared with those furnished in this table. It should, of course, be appreciated that the percentage of deaths from circulatory disease in these groups would naturally be low on account of the comparatively early ages and the recent physical examination.

III.

DEATHS FROM CIRCULATORY DISEASE ON BUSINESS WRITTEN

1904 TO 1910.

North Central States	27. %
New England States	26.83%
Middle Atlantic States	23.16%
Western States	23. %
Southern States	21.25%

Life Insurance Table No. IV. shows the percentage of deaths from circulatory disease which have occurred in the locations named. These figures may, perhaps, be somewhat qualified

by a question as to the accuracy of the medical examination in different locations and districts. It is not probable that a sufficient error has entered from this source to materially alter the figures; but whatever this effect may have been it is not probable that the medical examiners outside of New England have been so much more accurate as to account for their relatively low death rate from this cause.

IV.

DEATHS FROM CIRCULATORY DISEASE ON BUSINESS WRITTEN 1844 TO 1910.

Boston and Massachusetts	43.75%
New York and New Jersey	42.75%
Philadelphia and Eastern Pennsylvania	40.75%
Chicago and Illinois	26.26%
New England	41.75%
Southern Country Business	37.75%
Southern Cities	35.59%
Western Cities	35.25%

Taking the average on one year with another, insurance companies decline about 10% of those who apply to them for insurance. During the year 1910 of those persons who were declined by our company, 49.4% were on account of circulatory conditions.

It should be remembered that those who apply for life insurance almost invariably believe themselves to be in good health, and all of them give a sufficient impression of good health to justify their presenting themselves for examination; so that this table should be read with the understanding that 5% of all apparently healthy people, of the insurable classes, have at times such an impairment of the circulatory apparatus as to cause a declination by our company.

Insurance Table No. V. shows the experience of our company in the declination of applicants in the locations noted.

V.

DECLINATIONS FROM CIRCULATORY DISEASE

1910.

New York City and New Jersey	69%
Boston and Massachusetts	68%
Philadelphia and Eastern Pennsylvania	52%
Chicago and Illinois	47%
Group of Western Cities	45%
Group of Southern Cities	43%

Even a superficial study of the tables which have been supplied will demonstrate that New England, Massachusetts, and Boston occupy the unenviable position of leading the country not only in the mortality from circulatory disease, but in the relative increase in such diseases during the past few years.

It does not enter into my province, at this time, to even attempt to suggest the reasons for this condition, but I think that a careful study of the statistics furnished, with some consideration of the conditions found in the localities named, will be at least suggestive.

I believe that it will be helpful for us to recognize these conditions and to at least in part appreciate their gravity. I hope that such a recognition will bring to the minds of the medical profession of Massachusetts the necessity for seriously considering the possibility of controlling these diseases, as it has those more ordinarily recognized as preventable. I believe that while deaths from circulatory diseases must occur in every community, that it is perfectly possible for their number to be largely controlled.

The most serious problems which present themselves, to-day, to the medical side of life insurance companies are in connection with this subject of circulatory disease. These problems will never be settled until the medical profession as a body appreciates the gravity of the situation, the importance of its consideration, and the possibility of its control.

SYMPOSIUM ON CIRCULATORY DISORDERS.

ARTICLE XXVIII.

FACTORS INVOLVED IN THE PRODUCTION OF ARTERIAL BLOOD PRESSURE, PHYSIOLOGICAL AND PATHOLOGICAL.

By W. B. CANNON, M.D.
OF CAMBRIDGE.

READ JUNE 13, 1911.

THROUGH the walls of the capillaries the cells of the body are nourished and relieved of their waste. For the efficient activity of the cells these processes must be maintained without intermission. In order to force blood in a continuous stream through the capillaries all other functions of the circulatory apparatus exist. The main factor in assuring this continuous flow is the high head of pressure — a pressure varying between 100 and 200 mm. of mercury — which prevails in the arterial system.

The usual systolic arterial blood pressure—about 120 mm. of mercury — is much more than is actually necessary to drive blood from the arteries into the veins. This extra pressure has a function, however, which is seen whenever the arterioles of any organ or small area relax. If the arterial pressure were barely adequate to sustain a flow, the lowered resistance in such a region would seriously drain other regions. The high head of pressure, therefore, serves to keep all parts properly supplied with blood, even if an especially active part of the body is making an unusual demand.

The main factors determining the degree of arterial pressure are two: first, the energy of the heart, as measured

by the volume of blood pumped into the arteries in a given time; and second, the amount of resistance offered to the escape of blood from the arterial system during the same time. The resistance is met chiefly in the arterioles, whose walls of smooth muscle, in tonic contraction, permit either increase or decrease of the average size of the vessels.

Two other conditions—probably of minor importance—which may affect arterial pressure, are (1) the composition of the blood, and (2) variations in its volume. According to Burton-Opitz* the viscosity of the blood falls directly with the rise of temperature; it is less also after hemorrhage and during hunger, and increased by taking food, especially fatty and protein food. With increased viscosity a greater pressure is required to continue the rate of flow undiminished — such is the fact observed in fine glass tubes. To what extent, however, changes in consistency of blood affect its flow in the living body is not yet determined, though the evidence thus far obtained indicates that the effects are inconsiderable compared with the effects of vasomotor changes.

Variations in the volume of blood can have only a temporary effect, for very efficient means of regulating the blood volume quickly bring about the usual equilibrium. Thus, if an excess of blood is transfused into an animal, the liver stores it, transudation occurs into lymph spaces, secretion of kidneys and intestinal mucosa is increased, and dilatation of peripheral vessels keeps arterial pressure from rising. After hemorrhage all these processes are reversed; the liver gives up its storage, plasma is contributed from lymph spaces, the loss of fluid by the kidneys is checked, and contraction of the peripheral vessels helps to maintain arterial pressure.

It is noteworthy that both the viscosity changes and the volume changes of the blood can be compensated for in large

* Burton-Opitz: Arch. f. d. ges. Physiol., 1900, lxxxii, pp. 447-463, 464-473.

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degree by the interaction of the two main agencies already mentioned, which affect arterial pressure—the energy of the heart and the peripheral resistance. These agencies, and some conditions controlling them, are worthy of special attention.

THE ENERGY OF THE HEART.

As previously stated, the energy of the heart is measured by its discharge against arterial resistance. The amount of blood discharged will depend on several factors—primarily, of course, on the amount of blood that flows into the heart between contractions, but secondarily on venous pressure, on the degree of contraction, on the rate of beat, on the state of the muscle, and on other less important conditions. All these determinants of the cardiac discharge will now be considered.

The degree of cardiac contraction. In examination of hearts in heat rigor (which produces an exaggeration of the normal contraction), the ventricles have not been found wholly obliterated. In the apical end the walls are in contact, but near the base an open space remains.* Observations on hearts maximally contracted by strophanthin,† as well as records of hearts pumping in the body‡ support the same conclusion. Residual blood must therefore be present in the ventricle at the end of systole.

The amount of residual blood will vary with different arterial pressures. With very low pressure the ventricles may not empty themselves so thoroughly as when the pressure is higher, probably because the contractility of cardiac muscle is impaired by insufficient nourishment through the coronary circulation.§ As the arterial pressure rises and offers greater resistance the energy of ventricular contrac-

* Hesse: Arch. f. Anat., 1880, p. 328; Worm Müller and Sandborg: Arch. f. d. ges. Physiol., 1880, xxii, p. 408.

† Magnus and Loeb: Arch. f. exp. Pathol. u. Pharmakol., 1903, 1, p. 11.

‡ Roy and Adami: Phil. Tr., 1892, clxxxiii, B, p. 199.

§ Tigerstedt: Ergebn. d. Physiol., 1907, vi, p. 277.

tions, within rather wide limits, rises to a corresponding extent. This is probably due to two factors: to better circulation in the coronary arteries, and to the well known effect of increased tension in augmenting the ability of muscle to do work. Under these circumstances the ventricles will empty themselves to a maximum extent. If, however, the blood pressure in the arteries is sufficiently raised,* or if the volume of blood supplied to the heart is suddenly enlarged, the residual blood in the ventricles will again be greater than usual. Thus, powerful compression of the abdomen drives a flood of venous blood into the right heart, and quickly raises aortic pressure. Under these circumstances the ventricle will be overdistended in diastole, and will have difficulty in emptying itself. The inflow may be so great as actually to reduce the outflow. And if the heart muscle is weakened by disease, as for example, by certain of the infections, or by such drugs as chloroform and alcohol, the heart in the presence of the extra pressure may become so widely dilated in diastole, that the systolic output will be greatly reduced or become impossible.

With moderate arterial pressures, therefore, the emptying of the ventricles is performed most perfectly; and with very low or very high pressure the amount of residual blood will be increased.

The filling of the heart.—If, with moderate pressure, the ventricles almost completely empty themselves, clearly the amount of blood put forth by them will depend on the amount that flows into them. And, unless relaxation of the ventricles is hindered by external pressure, as, for example, by a paricardial effusion, the amount that flows in during a given time will depend directly on venous pressure.† The importance of venous pressure in determining

* Francois-Frank: Trav. de lab. de Marey, 1877, iii, p. 187.

† Howell and Donaldson: Phil., Tr., 1884, clxxxiii, p. 139.

cardiac discharge has recently been especially emphasized by Henderson,* who has designated it the "fulcrum of the circulation." Observations on man and lower animals indicate that normally venous pressure is sufficient to permit a rapid dilatation of the ventricles.

Records of the volume changes of the ventricles show that the greatest inflow occurs during the first stage of diastole, indeed that relaxation and refilling of the ventricles are equivalent procedures. After the ventricles have relaxed, however, an appreciable time passes before contraction occurs again. During this period, for which Henderson has suggested the name diastasis, the ventricles are in repose, and relatively very little blood is flowing into them.

The rate of the heart beat.—If the ventricles are supplied with blood at a uniform venous pressure, and if they almost completely empty themselves at each systole, does a change in the rate of beat affect the volume discharged? Numerous investigations have shown that cardiac systole varies much less in duration than cardiac diastole. Changes in heart rate, therefore, occur mainly through variations in the length of diastole. In the diastolic period the ventricles are filled from the veins. We might suppose that as the heart beats faster and the diastolic periods become shorter, less blood would enter the ventricles between beats; or, as the heart beats more slowly and the diastolic periods become longer, more blood would enter the ventricles between beats. Thus the changes of rate might be compensated for by changes in the volume discharged per beat, and in a given time the heart, whether contracting rapidly or slowly, might deliver the same amount into the arteries. Experiments prove, however, that acceleration of the heart rate results in an increase, and slowing results in a decrease, of the amount of blood delivered to the arteries in a given time. The explanation of this result is as follows.

* Henderson: Am. J. Physiol., 1910, xxvii, p. 158.

The faster beat is, to be sure, accompanied by a shorter diastole, but within wide limits the abbreviation occurs only in that part of diastole which has been designated the diastasis, *i. e.*, the period of repose, and does not affect the period of relaxation or filling. Only when the beat becomes so rapid as to encroach upon the period of rapid filling does the output fail to vary with the rate of beat.*

If peripheral resistance is considerable, therefore, arterial pressure will be altered in a direction corresponding to variations in the heart rate. If vascular tone is low, however, and the peripheral resistance is much diminished, even a great increase in the heart rate may not raise arterial pressure to any significant degree. Under these circumstances either the blood stagnates in the relaxed vessels so that the heart, without an adequate supply, cannot lift the pressure even though beating more rapidly; or the blood escapes so rapidly into the veins that the heart cannot, between beats, keep the arteries in a state of distention. In order that changes in heart rate may be effective on arterial pressure, therefore, a certain degree of peripheral resistance is essential.

THE PERIPHERAL RESISTANCE.

The peripheral resistance resides not so much in the capillaries, although the friction surface is there enormously increased, as in the small arterioles which bound the capillaries on the arterial side. This statement is supported by several observations. If, for example, the splanchnic nerves are severed, the blood pulsates directly through the capillaries into the veins—an impossible condition, if the capillary resistance were relatively great. Furthermore, the large drop from arterial to capillary pressure occurs most rapidly in the region of the arterioles, and only slightly in the larger arteries, a fact clearly indicating the region in which resistance consumes the energy of the heart.

* See Henderson: Am. J. Physiol., 1909, xxiii, p. 353.

With a constant output from the heart the arterial pressure will vary with the peripheral resistance. Under physiological conditions that resistance will vary with the degree of constriction of the arterioles. Thus high transsection of the spinal cord causes a complete relaxation of the vascular musculature and a profound fall in arterial pressure. Stimulation of the lower end of the severed cord will now result in a general vasoconstriction with a concomitant rise of pressure. Ordinarily the muscles of the arterioles are held in a state of moderate contraction, or tone. by a continuous stream of impulses sent to them from the vasoconstrictor centre in the medulla. This vasoconstrictor tone is the support of arterial pressure.

The peripheral resistance does not permit the blood to leave the arteries as rapidly as it is pumped in at each heart beat. The arterial pressure, therefore, rises with each beat. This is the systolic pressure. By the systolic discharge the larger arteries are stretched to receive the extra volume, but during diastole they contract again and push the blood onward into the capillaries and veins. As the arteries contract they exert less tension, and in consequence the pressure of the blood at the end of diastole is considerably lower than at systole. This low point makes the diastolic pressure. The difference between the systolic and diastolic pressures is the pulse pressure.

Below a certain limit the volume of excised arteries increases by equal increments when exposed to equal increments of internal pressure.* The maximum expansibility of the artery lies slightly above the usual systolic pressure. In other words arteries remain readily extensile up to pressures corresponding to ordinary variations in pulse pressure. At that stage the arteries can receive a relatively large amount of blood from the heart without undue increase of tension. Above that limit the cubic enlargement of the

* Roy: J. Physiol., 1881, iii, p. 125; 1888, ix, p. 227.

arteries is less and less with equal increments of internal pressure, until they become like rigid tubes. Then each cardiac discharge cannot be accommodated in an enlargement of the arterial capacity, but must be forced onward into the capillaries during ventricular systole. The work of the heart is thereby much increased, and the chances of damage to any weak or diseased vessel exposed to the heightened pressure are greatly multiplied.

Under normal conditions such dangerously high pressures are, of course, automatically obviated. As soon as tension begins to be too great in the beginning of the aorta or in the left ventricle, the depressor nerve is stimulated, whereupon impulses pass to the medulla and cause reflexly both a slowing of the heart-beat and an inhibition of vasoconstrictor tone. Thus both factors—the cardiac discharge and the peripheral resistance—are diminished in efficiency and the pressure falls. But in certain pathological states, to be mentioned later, this admirable conservative mechanism is not brought into action, and the pressure may then remain continously high.

Conditions increasing peripheral resistance.—Bodily states bringing about a heightened arterial pressure are both physiological and pathological. Among physiological conditions are age, posture, and muscular exercise. Several observers have reported that between the fifth and the twentieth year the arterial pressure nearly doubles,* and that there is also a further rise in the later years of life.† The change from horizontal to vertical posture causes almost no alteration in systolic pressure, but diastolic pressure rises. The mean blood pressure therefore is higher in the upright than in the prone position.‡ Exercise, as observations on

* Potain: La pression artérielle de l'homme. Paris, 1902. Zadeck: Ztschr. f. klin. Med., 1881, ii. p. 514.

† Albutt: Med.-chir. Tr., 1903, lxxxvi, p. 323.

‡ Erlanger and Hooker: Johns Hopkins Hosp. Rep., 1904, xii, p. 145.

dogs and men have shown,* causes an increase of arterial pressure. Indeed the increase in supreme efforts may be as much as 120 mm. of mercury.† There is some evidence that when doing the same work arterial pressure rises higher in old men than in young men.‡

Pain and excitement may perhaps be regarded as pathological states. Both result in heightened blood pressure. The stimulation of an afferent nerve causes a prompt rise in experimental animals, and the pain from irritation of the trigeminus has been observed to cause increased pressure in man. This fact is utilized in slapping and pinching the skin of a person who has fainted.

Excitement, especially that attending one of the major emotions, such as anger, rage, or fright, is accompaned by a marked augmentation of arterial tension. In these cases, as in the foregoing, the effect is doubtless mainly due to greater peripheral resistance, but the heart also may beat more rapidly and thus coöperate with the arterioles.

A very potent agent for constriction of the peripheral vessels is adrenalin. Even in very minute amounts—a little over a millionth of a gram—this substance is capable of producing a sharp, but temporary rise of pressure. Recently, Ascher has shown that by continuous injection of a very minute amount of adrenalin arterial pressure can be kept high for an indefinite period.§ This observation, taken in connection with the observation by De la Paz and myself‖ that emotional excitement produces an increased secretion of the adrenal glands, may have bearings on certain pathological states usually accompanied by high pressure.

Arteriosclerosis may or may not be attended by heightened arterial tension. Although rigidity of the arterial walls in-

* Tangl and Zuntz: Arch f. d. ges. Physiol., 1898, lxx, p. 544. Hill: J. Physiol., 1898, xxii, pp. xxvi–xxix.

† McCurdy: Am. J. Physiol., 1901, v, p. 102.

‡ Masing: Deut. Arch. f. klin. Med., 1902, lxxiv, p. 253.

§ Ascher: Zbl. f. Physiol., 1910, xxiv, p. 927.

‖ Cannon and de la Paz: Am. J. Physiol., 1911, xxviii, p. 64.

creases pressure at systole, in any particular case that effect may not be observed, for the sclerosis may involve so small a part of the entire arterial system, that the localized rigidity is readily compensated for by dilatation elsewhere. If the sclerosis involves the renal vessels, and nephritis of the chronic interstitial variety develops, greatly augmented blood pressure follows. This increased pressure is seen also in many cases of other varieties of renal disease. As Senator has shown in a recent analysis,* this rise of pressure cannot be attributed to mechanical interference with blood flow through the kidneys, but is probably due to some vasoconstricting substance circulating in the blood as a result of kidney disease. According to Schur and Wiesel,† the adrenalin content of the blood is larger than normal in these cases, but their evidence has not been substantiated.

Two other more acute pathological conditions which greatly increase vasoconstrictor tone are asphyxia and bulbar anemia. By making observations on arterial tension it is possible to tell when a developing cerebral pressure is beginning to encroach on the medullary centres.‡

Conditions decreasing peripheral resistance.—A marked fall of arterial blood pressure occurs whenever there is loss of muscular tone in the arterioles. Such is likely to be the case in acute poisoning by chloral or alcohol, both of which paralyze the vasoconstrictor centre. The collapse of the circulation that sometimes occurs in the course of certain of the infectious diseases, such as pneumonia and diphtheria, may be in part due to this cause. In surgical shock, again, the widespread abolition of vascular tone is accompanied by a very low blood pressure. In these instances the reduction may be due to a rapid pouring of the blood out of the relaxed vessels, or to a stagnation of the blood in the veins to such an extent that the heart does not receive a sufficient

* Senator: Ztschr. f. klin. Med., 1911, lxxii, p. 189.
† Schur and Wiesel: Wien klin. Woch., 1907, Nos. 23 and 40.
‡ Cushing: Am. J. Med. Sci., 1903, June.

supply to continue its normal discharge. Which of these conditions plays the more prominent rôle is not yet determined. Henderson has argued from evidence which he has accumulated that the condition of shock is attributable entirely to a failure of the supply of venous blood to the heart.

As is well known, vasoconstrictor tone can be diminished at will by use of the nitrites. A few whiffs of amyl nitrite produces a sharp drop in arterial pressure, which is, however, soon regained.

A physiological condition attended by lessened arterial pressure is sleep. Careful plethysmographic studies* have shown that as a person goes to sleep at night the peripheral vessels dilate, and that they remain dilated until the time of awakening approaches in the morning, when there is a general vasoconstriction. Blood pressure records prove that the period of general peripheral vasodilation is also a period of low tension.† Sleep, therefore, is not only a time of rest for the nervous system and skeletal muscle, but is attended by lessened work for cardiac and vascular muscle as well.

In conclusion, I wish to emphasize again the necessity of keeping in mind both the discharge from the heart and the constriction of peripheral vessels as determinants of arterial pressure. A low pressure may be due not to vasodilation, but to a weak heart; and any agency used to increase vasoconstrictor tone under these circumstances is likely to stop the heart at once. Indeed, my attention has recently been called to a case of precisely this character, in which the administration of adrenalin to raise the low tension was followed by instant death—the laboring heart was overburdened by the sudden increase of load and stopped immediately. Until the part played by each of the two factors, therefore—the heart and the arterioles—is clearly discriminated, the intelligent treatment of any disturbance of normal arterial pressure is impossible.

* Howell: J. Exp. Med., 1897, ii, p. 313.

† Brush and Fayerweather: Am. J. Physiol., 1901, v, p. 199.

ARTICLE XXIX.

REMARKS ON HIGH BLOOD PRESSURE AND ARTERIOSCLEROSIS.

By JAMES MARSH JACKSON, M.D.,
OF BOSTON.

READ JUNE 13, 1911

In a previous paper [1] written several years ago, I called attention to the importance of the routine measurement of blood pressure, citing as examples some cases in which, although the patient did not feel particularly ill, yet the presence of a high blood pressure led me to give a grave prognosis, the correctness of which was later established. Today I wish again to bring up that subject and to go into it a little more in detail in view of more extended observation of cases and of literature. And particularly do I wish to bring to your attention the great importance of recognizing that condition called by Huchard "presclerosis."

If we find a pipe-stem radial artery that is incompressible, and a ringing aortic second sound, in an elderly man with chronic interstitial nephritis, our diagnosis is made for us, but it is not that sort of case that I am dealing with, but a more subtle one, showing none of those characteristics, but on the contrary no change in the palpable arteries, a hardly noticeable increase of the aortic second sound and possibly a faint systolic whiff, and a normal or practically normal urine. When, however, we test the blood pressure with the Riva-Rocci manometer or one of its modifications, we find it to be 160 or 180 or even 250 millimetres of mercury.

In persons entering upon or early in middle age this increased blood pressure is—in the absence of nephritis—usually the only sign of a chronic poisoning arising from some deficiency of elimination, either intestinal or urinary.

So many of these cases come to our attention when we keep our eyes open for them that we are soon persuaded of the necessity for putting them in a class by themselves—cases in which we find one invariable sign, increased blood pressure, but are unable to detect as a reason for it any of the recognized and common causes for this condition.

Janeway[2] found in 13% of 130 cases showing a pressure of 200 mm. or more, no evidence of nephritis or of cardiovascular disease aside from the blood pressure reading.

Increased blood pressure *per se* is not necessarily objectionable. Most of us, when we find an increased blood pressure, immediately endeavor to reduce it, which is sometimes a mistake, for in a great many cases this increase is physiological—an attempt on the part of nature to force the necessary amount of blood through narrowed arteries to the brain and other important centres, and thereby keep up nutrition, or to force more blood through the kidneys and thus eliminate toxins circulating in the blood stream. If there is evidence of a failing competency of the heart, ease up on its work by very small doses—one two-hundreths—of nitroglycerine, and strive to find and eliminate the source—probably intestinal, at least dietetic—of the toxins which are causing this increase in pressure.

In general when we think of heart disease we immediately connote "murmurs." When, however, we stop to think we realize that many a person lives a long, active and useful life with severe heart lesions and loud murmurs, but in this case the heart muscle is strong and probably is not constantly bathed in toxins—in other words the heart is competent.

Another heart, however, without valvular lesions but the

muscle possibly not quite as strong, to start with, is exposed to the pernicious influences of excesses of work, food, worry, etc., and loses its competency. This condition in its slight degrees is very hard to diagnose; and yet just this time when the symptoms and signs are slight is the time at which to recognize it, and by regime and drugs check its further advance and perhaps restore the heart to normal if we be fortunate. The heart sounds must be listened to with the greatest care, an especial diligence being exercised to note any systolic roughening or increase in the second sound at the aortic area, the size of the heart must be determined by percussion or by x-ray, and the heart's competency to stand extra strain tested by noting the effect of exercise on the heart beat. Is the rate increased, or is a slight irregularity produced, or does it cause undue dyspnœa?

Again, as regards competency of the heart, it is not requisite that the blood pressure be high to produce a gradual wearing out of the heart from overwork, for a blood pressure of 100 mm. may be just as great a burden to a very weak heart as a pressure of 190 mm. is to a stronger one. A heart of this sort needs two things, relief and slight stimulation, *i. e.* small doses of vaso-dilators and gentle tonic treatment by digitalis.

Furthermore, the blood pressure does not need to be very much increased to injure any ordinary heart in time, for the amount of work that even a few millimetres of increased pressure adds to the heart is amazing. Sheldon [3] calls our attention to this fact by remarking that, granting the normal systolic and diastolic pressures to be 130 and 90, if we find corresponding pressures of 182.5 and 108 the heart overworks to the equivalent of 39.2 mm. of mercury at every beat. Multiplying this by a rate of 70 beats per minute we find that this heart will do almost 4000 *metres* of extra work—still in terms of the same mercury column—in 24 hours.

"The attempts to establish a working hypothesis," says Elliott [4], "to account for all the varieties of hypertension, leads us face to face with the toxic theory. Arterial hypertension is best exemplified as it occurs in a group of chronic diseases having as their common essential characteristic, toxemia. In all probability the *materies morbi* consists of certain biochemical products present in the circulating blood. This is apparent in scarlatinal nephritis in which the tension rises a few hours after the appearance of albumin in the urine, entirely too early for the rise to be explained by the formation of arterial fibrosis. The high tension of uremia is another instance in point. For laboratory proofs we have the well-known fact that the pressor principle of adrenal and pituitary glands, and also certain drugs (ergot, nicotin, digitalis) will raise blood pressure."

The source of these toxins—eliminating the nephritic element—is always very difficult and in many cases impossible to ascertain. Clinical experience shows us that frequently the intestinal tract is at fault either through frank constipation or slow and deficient elimination—"delayed traffic." Long periods of putrefaction of nitrogenous foods give rise to toxic substances which are absorbed and exert a pressor effect on the blood vessels.

As an instance of this let me cite the following case:—

Woman of 43. Never any serious disease. For the past ten years has been subject to frequent severe attacks of headache, and to profuse nosebleeds which relieved the headaches. The appetite has been good and the bowels have always moved daily. During these ten years she has been under treatment by several good men for "neurasthenia."

Physical examination shows the heart enlarged to the right and to the left. A slight systolic murmur at the aortic area. The aortic second sound accentuated. The arteries not palpably sclerotic. The urine shows nothing abnormal. But the blood pressure was 230 when I saw her in one of her attacks of headache.

Now, though there had been a daily bowel movement, we investigated the conditions there and found that irrigation of the colon brought forth a large amount of mucus and evidence of great putrefaction, and from a single dose of medicine we obtained eight large movements, proving the presence of stasis—or "delayed traffic."

By daily irrigations of the bowel, laxatives and a diet of vegetables and fruits, the headaches were entirely eliminated, the pressure fell to 170 and the heart has returned practically to normal size. From an almost intolerable condition the patient is now in apparently perfect health, though the pressure remains at 170, and the aortic second sound is still somewhat accentuated. This is owing to the presence of some arteriosclerosis secondary to the long continued toxæmia and high pressure.

Other cases that I have seen have left no doubt in my mind that long-continued overwork is a potent cause of high tension, and its sequel, arteriosclerosis. We see it in business men, in lawyers, in ministers, in doctors—in all walks of life, but particularly among brain workers who are steadily kept at a high pitch. I am sure that there is a tremendous waste of the best material in the community caused by our American custom of constantly exceeding nature's speed limit and too rarely laying up for repairs and overhauling.

Take as an example this patient:—

Man of 49. President of a large trust company with heavy responsibilities. Has always been well and strong, but for thirty years has devoted himself entirely to his business and in the past twenty years has not had a day off. No alcohol or tobacco. Tea and coffee very moderate. Has always been a hearty eater, as was necessitated by the energy he consumed. For six months he has been under treatment for "neurasthenia" on account of occipital headaches and inability to concentrate his mind. But we find on going deeper into his history that he has slight dyspnœa on walking, attacks of precordial distress, and severe nose bleed two or three times a week. On examination we find that this man who has recently been told that he was physi-

cally sound, has a heart apex one half inch outside his nipple line, an accentuated aortic second sound, a typical chronic interstitial nephritis, and a blood pressure of 230.

I feel that we attribute to arteriosclerosis more than its share of blame for raising blood pressure. This probably arises from the fact that we can find at least thickening of the vessel walls in every case where the blood pressure has been elevated for any considerable time, and we jump at the conclusion that the increase in pressure is caused by the sclerosis, when as a matter of fact the contrary is the case.

That we can have very marked arteriosclerosis with little or no elevation of the blood pressure I have observed in many cases, and my observation is substantiated by Rudolf who in an extensive investigation found that in only 50% of cases of well marked arterial thickening was the blood pressure greater than normal, and he mentions several fatal cases of sclerosis in which the blood pressure was not increased.[5]

The causes in general of arteriosclerosis can be summed up under the heads: (1) Toxic—*e. g.* lead, infections of all sorts, excess of certain internal secretions such as adrenalin, and absorption from the intestinal tract of certain putrefactive products, and (2) Senile or quasi-senile, in which would be included the wear and tear from over-work.

As to the first group, the toxic, lead and syphilis have long been recognized as prominent factors in the production of arteriosclerosis; clinical findings in cases of adrenal tumor and experimental work has shown that long continued administration of adrenalin brings about the same result. Lately many infections, such as influenza, tonsillitis, peritonitis and others are being held responsible for arterial changes. Marchand[6] for example, quotes many cases of persons under 30 in whom arteriosclerosis was found at

autopsy. No primary nephritis was found in these cases, and the etiology of the arterial disease when noted is in most of the cases infectious, diphtheria, rheumatism, scarlet-fever, measles, typhoid, etc. The work of Metchnikoff has shown without a doubt the rôle of the intestinal putrefactive products in producing sclerosis, particularly the so-called "aromatic series." In his laboratory many of these substances which are found in the urine of man were injected into animals—rabbits, guinea pigs and monkeys—in small doses over a long time, and the results collected. It was found that when paracresol was used, at the end of two months or more there was arteriosclerosis in ten times as many of the animals as there was in the untreated control animals. Indol was another substance which was found to produce marked sclerosis, while skatol was harmless. These substances are produced in the intestine in considerable amounts by bacterial action, and are cumulative in their effect upon the blood vessels, to which they gain access through their absorption by the intestinal mucous membrane [7].

Of the second division, that of wear and tear, I have already spoken sufficiently.

There is also another factor in the production of arteriosclerosis, which should perhaps be given a separate heading, that is, heredity. As Broadbent [8] says: "In no class of cases is it more necessary to inquire into the family history than in diseases of the heart," and to this we can add "and arteries." He also mentions [9] the frequency of hereditary high arterial tension, and I have seen several instances of an undoubted hereditary tendency to arteriosclerosis. For example, two children of a tabetic father, one of whom has had cerebral hemorrhage and the other coronary sclerosis and angina pectoris, and both of them are under 40.

Romberg [10] made a careful search for arteriosclerosis in

his clinic in Marbourg and found it in a surprisingly large percentage of his cases—in 14% of males between 30 and 39 years of age, and in 29% of males from 40 to 49. Furthermore, Klemperer[11] reported a few years ago that in the last three years he had seen fifty-one doctors between the ages of 28 and 40 who showed marked arteriosclerosis.

With regard to the symptomatology of increased blood pressure and early arteriosclerosis, whole books might be written, for the symptoms may simulate almost any known disease and cause nearly anything from a fleeting dizziness to gangrene of the lower extremity, while on the other hand, in a spite of a very high blood pressure, the patient may feel no discomfort and be led to no wise to diminish his activities. In most instances the change in pressure has taken place so slowly that the body has had a chance to adapt itself, *pari passu*, with the gradual increase and no notable upheaval is brought about.

Some of the early symptoms I have already mentioned, and to these I might add several that are frequently attributed to neurasthenia, such as vague unpleasant feelings in the head, slight dizziness, cold hands and feet, unrefreshing or disturbed sleep or sleeplessness, gastric distress and flatulence, usually one hour or so after meals, irregularities of the bowels, especially constipation. Later the patient complains of lacking initiative and the power of application. Furthermore the inhibitory centres become weakened and tolerance to substances which affect the brain—such as tobacco and alcohol—is diminished and the patient tires easily.

Soon the gastric symptoms increase in severity, and any exertion after a meal brings on a severe attack of pain which is relieved only by resting, and yet the patient is treated with bicarbonate of soda, anti-fermentatives, laxatives and what-not, when the source of the trouble is in the sclerosing splanchnic vessels and a weakened heart muscle.

All these symptoms are such as usually bring the patient

directly to the internist, but there are others that carry him first to the specialist on account of local disturbances. Let me cite as an instance of this, the case of a woman of 59, always well and strong, who had been for two years under treatment by an aurist for deafness and constant tinnitus aurium, but without relief. Finally in despair the aurist sent the patient for a medical "overhauling," to see if by any chance there were any cause outside the ear for the symptoms. On examination the patient's heart was found to be enlarged, there was a systolic murmur at the aortic area, the urine showed a chronic interstitial nephritis and the blood pressure was 250 mm.

On a diet which entirely excluded meat, and with very small doses of nitroglycerine, the pressure fell rapidly to 170, the heart diminished considerably in size, the tinnitus and deafness almost entirely disappeared, and the patient has remained in good health and perfect comfort for two years.

Not infrequently the patient gets his first hint of his condition from the ophthalmologist to whom he has gone because of failing vision, either gradual from simple atrophy, or sudden from retinal hemorrhage.

De Schweinitz in an excellent paper[12] has described the changes in the retinal vessels in persistent high arterial tension when a symptom of arteriosclerosis, and has emphasized the value of ophthalmoscopic examination in determining the condition of the arteries, even at a stage of the disease so early that no general symptoms conclusive of arteriosclerosis have appeared. He says: "Eye ground determination . . . may render signal aid in the interpretation of symptoms caused by the derangement of the functions of important organs, which in their turn are dependent upon sclerotic changes in their smaller vessels, although there is as yet no decided alteration in the general circulation." Furthermore, he strongly urges ophthalmologists to

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refer patients to the internist for investigation and treatment when sclerosis of the retinal vessels is found, particularly since his experience has taught him that the condition of the retinal vessels is very strongly indicative of the condition of the cerebral vessels, and by taking the hint and beginning treatment early, apoplexy may be avoided and life prolonged.

As I have said before, we may for a time suspect no abnormality of the blood pressure or of the vessels in those in whom on careful examination we find it most marked. Some of them are pale and pasty and clammy-handed because of the narrowing and sclerosis of the arterioles in the skin; but others are ruddy, bright-eyed, active, high strung and constantly "on the go." They are often looked upon as our most brilliant citizens—our most brilliant conversationalists, the most tireless dancers, our most energetic workers in every good cause, or our Napoleons of finance. Why are they so brilliant and tireless? Because blood is being forced to their cerebral centres at a pressure of 200 mm., the hyperemia stimulating them to do an enormous amount of work.

On going more deeply into their history, however, we find that they have periods of great depression, that they are extremely irritable and have terrifying nosebleeds, that they have severe headaches and nausea, or sudden vertigo. Why is this? Because of the irritation of their cerebral centres by the continued high pressure; and it is interesting to see the degree to which their symptoms are relieved by a profuse nosebleed and resultant lowering of pressure.

After a while their periods of depression become more and more prolonged. They lose strength, their memory becomes unreliable, business cares become a great burden, and an ordinary day's work becomes an almost insurmountable obstacle, they begin to have constant indigestion and sleepless nights,—in other words they present the picture so often seen in neurasthenia.

Often it is not until these people have hemorrhages in the retina or show symptoms of cardiac weakness, or complain of frequency of urination at night, that they are suspected of having anything more than a nervous condition. Then it is found when almost too late that they are suffering from early arteriosclerosis, that their blood pressure is 200 or more, or that their urine contains albumen and casts. Then only, is a true interpretation put on the symptoms of which they have complained. I say "too late," because it means a year at least of absolute rest, freedom from care, an out-of-door life, with restricted exercise, and a strict adherence to a carefully planned non-nitrogenous diet before they can return to anything like normal health. But even then they are "damaged" people; they can never return to their work; can never again live at the high speed of former years, but must descend to a much more moderate life or prepare for the hereafter.

A few minutes ago I quoted some figures showing the prevalence of arterio-sclerosis in persons in early middle life. How many doctors are there just on the shady side of forty who have done a large and trying practice who suddenly develop symptoms of neurasthenia—who become nervous and irritable, worry over their cases, and find that in order to do their work they must have a certain amount of stimulation?

These men have been living two years in every one, and have been burning the candle at both ends. I find not infrequently that these men are carrying blood-pressures of 160 to 180 mm., and that they are proud of the fact that for the past ten years they have never once taken a real vacation.

These men are not neurasthenic, although they will pester the life out of their consultant about this or that nervous symptom in their stomach or head. Those men are worn out, their kidneys, their arteries, their brains have been

pushed to the limit, they have never had enough sleep,—their snap is gone, and it is then that they seek advice for "that pain in the back of the neck," or sleeplessness, or because they are worried about some trivial ailment.

It is because we see so many of these cases that I am making a plea to-day for the earlier recognition of increased blood pressure and its sequel, arteriosclerosis.*

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SYMPOSIUM ON CIRCULATORY DISORDERS.

ARTICLE XXX.

TREATMENT IN CIRCULATORY DISORDERS.

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OF BOSTON.

READ JUNE 13, 1911.

THE limitation of time will not permit an exhaustive discussion of the subject of the treatment of circulatory diseases. We can neither consider the treatment of all the diseases properly coming under this head nor all of the therapeutic measures that may be useful. Much less can we consider systematically the treatment of all the numerous symptoms. It seems best, therefore, to omit from consideration in this paper the treatment of common diseases of the heart, kidneys, and blood vessels, and of the classical symptoms of these diseases. These conditions, where one of these organs is definitely diseased, are commonly recognized by the profession, and the methods of treatment are pretty well understood. It will be more profitable to devote the time to the consideration of certain conditions of the circulation which are not so commonly recognized, where the efficiency of the circulation is impaired without marked disease of the individual organs. Many of these conditions are imperfectly understood because they are not properly studied by practitioners. As a result they are poorly treated, and one becomes pessimistic as to the benefits to be derived from treatment. On the other hand, careful study will often lead to a thorough comprehension of the condition

in question, and this makes possible the intelligent direction of treatment, from which such gratifying results often follow that pessimism gives place to optimism.

We may say in general that while drugs have an important rôle to play in the treatment of circulatory disturbances, their value has been much over-estimated. The secret of success lies in early diagnosis,—in prevention rather than cure; in regulation rather than in medication; in diet rather than in drugs. And yet much may be done for advanced cases also,—even for cases commonly regarded as hopeless. If we carefully estimate the functional power left in the impaired organs and adapt the patient's life and habits to the limitations thus ascertained, we can sometimes secure an adjustment of functions that is equivalent to relative health.

But even such measures as are here indicated cannot be exhaustively treated in a short paper. I shall, therefore, attempt to consider only certain aspects of the subject which have impressed me as having a special value in treatment. If certain other measures are not as thoroughly treated, it does not mean that they may not be equally valuable, but merely that the writer naturally dwells upon those measures that in his experience have given satisfactory results. It is true of this subject, as of many in medicine, that certain desirable physiological results may be attained by several different therapeutic means. The main thing is to secure the results. The physician may choose such of the available measures as his experience and opportunities enable him to use to best advantage.

Some of the problems we have to consider can best be illustrated by a concrete case. The following is a typical example. A lawyer, sixty years of age, came to consult me in March, 1908. He was what is commonly called a "cardio-renal" case, and showed evidence of chronic interstitial disease of the kidneys, considerable arteriosclerosis,

and a failing heart due to myocardial degeneration. He had been complaining of symptoms since the previous October, and had consulted three physicians in succession, and had been treated by them, but without relief. Each physician had in turn assured him that there was "nothing the matter with the heart," and his troubles were ascribed to overwork, nervousness, and disturbance of digestion. But general tonics and stomachics failed to relieve. He decided that there was trouble with his heart in spite of the opinion of these physicians, and persisted in his search for relief along this line.

In this instance the layman was right in his diagnosis, and the three physicians were wrong; and yet all three stood well above the average of the profession in education, in experience, and in professional reputation. Such failures on the part of good physicians to definitely recognize certain diseased conditions of the circulation are sufficiently common to warrant me in dwelling a little on the importance of diagnosis in these cases, notwithstanding the fact that my proper subject is that of their treatment. The trite saying, that the first step toward proper treatment is a correct diagnosis, has nowhere a more appropriate application than in the case of diseases of the circulation. By a correct diagnosis I do not mean a haphazard statement that the patient has "cardio-renal disease," but an intelligent analysis of the disturbed conditions of the circulation that need to be remedied. This subject has already been considered by others in this symposium, yet it seems to me of sufficient importance to warrant reiteration. In fact, success in the treatment of these conditions depends chiefly on the thoroughness with which the problems of each case are studied, next upon the intelligence which directs the application of remedies, and only to a lesser degree upon what remedial measures are employed.

Let us study for a moment why the three physicians

failed to recognize the condition of the patient under consideration. His family history was exceptionally good, and he had enjoyed unusually good health up to his present illness. He was a man of excellent habits, and led a methodical, well-regulated life. The commoner etiological factors were lacking, the indications of disease to be found in the physical examination were either very slight or required more accuracy in technique than these physicians were accustomed to use, and they failed to give due consideration to the history of his subjective symptoms for several months. So, failing to make a diagnosis, they tagged him with the name of "nervousness," "debility," "stomach trouble," or any other general name that will serve as a cloak for ignorance.

This man had been assured a short time before he saw me that there was no trouble with his heart, and yet he had disease of the circulatory organs and kidneys so far advanced that within a week from the time I saw him he suffered from what may fairly be called uremic vomiting, and had his first attack of cardiac asthma. Are you inclined to severely condemn these physicians for failing to recognize this advanced condition of disease? You would be unjust if you specially condemn these individuals, for they were above the average of the profession; they gave as much time, study, and thought to this case as the average physician would, and they merely did not happen to "hit it" right. Condemn, if you will, the faulty, inaccurate, and superficial work of the average physician, and I fully agree with you; but do not single out these three individuals who are no worse than hundreds of others. However adequate these methods may be for dealing with the average case of slight or of well-defined forms of illness that form the bulk of general practice, these methods are utterly inadequate for the proper recognition of many diseased conditions of the organs of circulation. Consequently more than the ordinary

care, study, and thought are demanded, if these conditions are to be successfully treated.

Yet it often happens that expert skill is not needed for a satisfactory solution of the problem, and the mistake comes largely from failure to employ intelligently the knowledge and means of investigation that are easily within reach of the general practitioner. Such was the case with the patient we are studying.

In this case it was not creditable to disregard the statements of an able lawyer with a keen, analytical mind. His observations were unusually good and accurate. Neither was it creditable to ascribe his symptoms to "nervousness," when he had the calm, phlegmatic temperament of the patient plodder. Not much more credit was due for ascribing to "debility" a trouble that arose just at the end of a rather long summer vacation spent under favorable conditions for recuperation. When a man of this type says he has dyspnea and palpitation, it almost certainly means some definite trouble with the circulation. When you learn that the symptoms first appeared after unusually severe exertion for him, in preparing his summer place for the winter, and that the symptoms appear only with exertion, you are warranted in a definite conclusion, that his heart is inadequate for reasonable demands of the circulation,—whatever may be the findings of the physical examination of the heart. I would emphasize the fact that in studying the condition of the circulation the careful consideration of symptoms is often of even greater importance than a careful physical examination.

In this case the history, combined with the observation of slight but definite breathlessness on slight exertion, and slight puffiness of the tissues of the face, and definite puffiness of the eyelids, warranted a provisional diagnosis of advanced cardio-renal disease, with a failing heart, before the patient was touched in the physical examination.

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The first touch gave confirmatory evidence in a high-tension pulse, and added the information that there was definite arteriosclerosis. Next, the measurement of the blood pressure showed that it was 210 m.m., and our case is practically complete. We can confidently predict that the urine will show signs of interstitial nephritis, and that the heart will be found enlarged, and not very strong. These conditions were clearly demonstrated by proper examination.

It should be said, in fairness to our three confreres, who failed to recognize these conditions, that the test for albumen showed only the faintest possible trace, and that it would be missed in the ordinary test as performed by the average physician; that the casts were found only by letting the sediment settle and then centrifugalizing it; and that the patient was fat enough to make the determination of the size of the heart difficult by percussion. Perhaps I ought to add that there were no cardiac murmurs, although their presence or absence may have nothing to do with the adequacy of the heart in maintaining the circulation.

Of all the omissions and mistakes of these three physicians, the most vital one was the failure to determine the blood pressure. It could have been easily determined by the use of apparatus, and by the touch alone it could be easily recognized to be distinctly high. If a physician does not make the instrumental determination of blood pressure a part of his routine examination, he should at least make the estimation of the tension by palpitation as essential a part of his physical examination as he does the rate of the pulse. Rough though the estimate be, it will save many grievous mistakes. One ought, however, to add a word of caution, to the effect that the height of the blood pressure alone does not measure the gravity of the case. A high pressure for a strong heart may not give nearly as much trouble as a relatively low pressure causes a weak heart.

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The determination of the blood pressure in this case, as in many of these conditions, settled the general line of treatment. Our patient was suffering from a circulatory condition in which a relatively weak heart was struggling against a high pressure. Under these conditions cardiac tonics are advisable, to strengthen the heart action. My preference is still for our old friend's digitalis and strychnia. However, they should be used cautiously in these conditions, and often in very small doses.

It is well recognized, of course, that digitalis or its preparations do not agree with some cases of myocardial degeneration. Consequently it must always be used with caution in this condition. I do not think we can yet satisfactorily explain why it does not agree with some of those cases, nor can we always tell beforehand whether it will agree or not. However, I have been more and more impressed with its usefulness in these cases, if the amount is properly adjusted. It is rare that two or three minims (or five drops) of the tincture, repeated twice a day, will do any harm, and it is safer not to begin with more than five minims (or ten drops) three times a day. Remember that it is relatively slow of absorption, and consequently its effects are not quickly realized. Ordinarily in a day or two we can tell whether its effects are to be good or bad, and whether the dose is sufficient to produce results. In my opinion, it is well to stop at a dose that is producing recognizable improvement, rather than to increase the dose in the hope of securing more rapid progress. It is easy to give too much, and owing to its slow elimination, any undesirable effects that it causes may last some time. Because its action persists for some time on account of slow elimination, it is almost always safe, when you suspect the dose may be too great, to omit it for a time and watch the results. If conditions become better, you were giving too much; if they are not as good, you are warranted in cautiously in-

creasing the dose. In some cases the dose must be increased considerably before favorable results are obtained. This is especially true when there is present a relatively recent dilatation of the heart.

The limits of my paper will not permit a discussion of the merits of different preparations of digitalis, nor the variability of the strength of the same preparation as obtained from different sources. However, I do wish to say that, if the action of the drug is studied intelligently in a case, you can generally get satisfactory results from any standard preparation obtained from a reliable druggist. I am aware that experiments show considerable variation in the strength of these preparations, yet in any supposedly reliable preparation there is sufficient strength to get results if the dose is properly adjusted. Our present knowledge of the chemistry and physiological action of various preparations from digitalis is so unsatisfactory, and our knowledge of its exact action in different circulatory conditions is so imperfect, that the use of this drug in any given case is now essentially experimental rather than scientific. While variability in strength is undesirable, after all it adds but one more uncertain factor that must be tried out experimentally in a given case. I do not wish to be misunderstood. I am in favor of securing uniformity in the strength of digitalis preparations as far as possible, and the best attainable is none too good; but I do wish to combat the idea that you must have a preparation of a given strength in order to get any results and that nothing else is useful. Clinical experience does not warrant this deduction.

Objection is sometimes made to digitalis in these conditions under the impression that it increases the blood-pressure. In my experience this does not occur from small or moderate doses of the drug. When the blood pressure is low because of lack of power in the heart-muscle, we may get a considerable rise in the pressure as the strength of the heart increases, but this is a desirable change.

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Strychnia is a very useful drug in these conditions where better cardiac action is desired. Here, again, moderate doses have no effect in raising the blood pressure, unless it is low because of cardiac weakness. I rarely use it in doses larger than 1/60 grain at the beginning, and often in doses of 1/100 grain, or less.

Returning to the problem of the treatment of the case that was under consideration, it was very desirable to reduce the blood pressure. The commonest measure employed to secure this result is the use of vaso-dilator drugs, like nitroglycerin. Sometimes very gratifying results are obtained in this way,—at least for a time. Often, however, the action is transitory or no appreciable improvement is secured. As the action of the drug is rather fleeting, it should be given at frequent intervals, and preferably in as small doses as will produce any appreciable result. When it is needed at all it should be given as often as once in three hours. The dose should be 1/100 grain, or less.

The effect of nitroglycerin on the blood pressure—as we measure it—is usually lost in about half an hour. Yet I cannot believe that this tells the whole story of its usefulness. The beneficial effects certainly last longer than that in many instances, and one can certainly detect a different quality in the pulse for two or three hours, although the systolic blood pressure, as we determine it, has returned to its former high level.

Nitroglycerin, in my experience, rarely lowers the blood pressure more than 10 m.m. for any length of time. Even this small gain is often a great help to a laboring heart, and this drug (or one of the other vasodilators) is often a most valuable aid in treatment. It is often useful even when the blood pressure is moderately low, if it is low because the heart is failing.

The cardiac tonice and the vaso-dilators comprise the drugs that have a directly useful effect on circulatory dis-

orders of the type that we are now considering—where a weak heart is struggling against a blood pressure that is high as compared with the heart's strength. Let us now turn to non-medicinal measures.

Of cardinal importance is rest from physical exertion. This must be more or less complete, according to the needs of the case. Little need be said on this point, for the need of physical rest is generally recognized by the profession. Freedom from mental activity and worry is less generally recognized as necessary, but it is of great importance. The diet should be light and easily digested, but must contain sufficient nourishment, otherwise it is impossible to build up a strong heart. The bowels must be regulated so as to secure free elimination from the alimentary canal.

Hydrotherapy is not useful in this condition. The effect of baths that are either cold or hot is generally to raise the blood pressure. Especially is this true of the Nauheim baths. These baths are so useful for certain chronic cardiac conditions that a word of caution is needed, about cases where they are not suitable. Hinsdale summarizes the matter as follows: * "A positive counter-indication to such measures as the Nauheim bath, for instance, is any acute or subacute condition of the heart, or when, on the other hand, in chronic heart disease compensation is lost." On the other hand, in cases where there is at least fair compensation the effect of such baths is beneficial. There is improved assimilation and tissue metabolism, the area of a dilated heart is diminished, its strength is increased and there is a distinctly beneficial effect on the nervous system."

In 1908 the writer published an article on "High Frequency Electricity in the Treatment of Cardiac Disease." † In this article he called attention to the effect of high-

* Hinsdale. Practical Treatment, Musser and Kelly; Vol. 1, p. 458.

† Transactions of the American Climatological Association, 1908. Boston Medical and Surgical Journal, Aug. 20, 1908. Vol. CLIX, pp. 233-238.

frequency electricity in reducing blood pressure. A reduction of 10 to 20 m.m. at a treatment is common, and a reduction of 30 or 40 m.m. by a course of treatment is frequently obtained. Contrary to what might be expected the resulting improvement may last for weeks or even months. Further experience has served to increase confidence in the usefulness of this measure in the treatment of these cases.

But, in the writer's opinion, the most important factor in the treatment of these cases is the proper regulation of the diet. Far more striking results have been obtained in this way than in any other. The essential feature of this dietetic treatment is the limitation of the amount of protein in the food. It seems a fair inference from the experience with such cases, to conclude that the waste products of nitrogenous metabolism have a distinct effect to increase the blood pressure. How far this is due to the effect of these substances on the nervous system, and how far to the effect directly on the heart and blood-vessels, is less clear. Very possibly they have an effect on both. It is a fact, however, that a sufficient lowering of nitrogenous waste products are potent factors in reducing the blood pressure, for the pressure falls if the protein intake is so regulated as to be below the amount that the kidneys can eliminate. With such an adjustment an accumulation of these waste products in the system fails to occur.

I am unable to tell you just what these chemical substances are which cause this trouble, and apparently the physiological chemist is not yet prepared to enlighten us on the subject. It is better, then, not to theorize as to their exact chemical nature. This is of less practical importance compared with the demonstrable fact that in many, if not in most cases, a fall in blood pressure follows the application of this method.

If our explanation of the way in which this therapeutic

measure acts is correct, it should commend itself all the more to us because it removes the cause of the trouble instead of attempting merely to correct or alleviate the resulting symptoms. Hence its advantage over all the other measures enumerated above. It commends itself to us again because of its effectiveness. A drop of 40 m.m. is common, and 50 m.m. is not unusual. The drop may reach 60 m.m. or even more. It has worked thus efficiently in many cases where the drug treatment failed to produce any appreciable result. Another advantage is its permanency, for it can be carried on indefinitely without injury and with only benefit to the individual.

A word of caution should, however, be inserted here. While it is desirable to lower the amount of protein in the food, it is absolutely essential to keep up a sufficient supply of nutriment. In more than one case I have seen undesirable results when the patient, though willingly foregoing the desired amount of protein food, would not take enough nourishment of other sorts to maintain the strength of the heart. The result was a further weakening of the heart muscle and an increase in symptoms.

I cannot go into the details of this treatment at the present time. The method of regulating the food was the subject of a paper which I read before this Society in 1909,* and the application of these methods to the treatment of chronic nephritis was the subject of another paper read before the Medical Section of the American Medical Association in June, 1910.†

I would not be understood as offering this treatment as a panacea for all conditions of this sort, nor as asserting that it will always be successful in reducing the blood pressure. When the renal inadequacy reaches a certain limit it is of

* A Practical Diet Chart. Transactions of the Mass. Medical Society, 1909. Boston Medical and Surgical Journal, Sept. 30, 1909, p. 457.

† The Importance of a Low Protein Diet in Chronic Nephritis. Jour. Am. Med. Assn.; Dec. 24, 1910, Vol. LV, pp. 2193-2197.

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no avail. In some cases, though working satisfactorily at first, it has failed to relieve when the disease of the kidneys has advanced farther. But it has accomplished surprising results in many cases so advanced that they would ordinarily be considered beyond help. In other cases, where it has ultimately failed as the disease advanced, it has prolonged life and comfort over a period of months at least.

The case which I took as an example in the earlier part of this paper, illustrates the importance of a fairly accurate regulation of the amount of protein. The protein food had been cut down roughly from the start; drug treatment and high frequency electricity had been called in to help; and yet in the year following his first attack of pulmonary œdema on March 18, 1908, he had twenty-two attacks of pulmonary œdema, varying from mild to very severe. These attacks were increasing in severity and frequency when, in March, 1909, the diet was carefully regulated quantitatively as to the amount of protein it contained. His attacks of pulmonary œdema ceased, he regained a degree of health almost as good as before his trouble began, and he lived a life of comfort, usefulness, and moderate physical activity until in May, 1910, he succumbed to an acute pulmonary infection, which also developed pleurisy with effusion and pericarditis. The accurate quantitative regulation of the amount of protein prolonged his life for fourteen months, when his condition was such that he could not reasonably have been expected to live more than a month or two longer.

Once more I wish to state that the dietetic treatment of these cases is not a panacea, but only one very important therapeutic measure among a number that are useful in diseases of the circulation accompanied by a blood pressure that is high in comparison with the efficiency of the heart.

I have dwelt at some length on the benefits to be derived from the proper dietetic treatment of advanced cases of this

sort. We should expect to be warranted in concluding that the same method of treatment would be useful in earlier stages of this disease. The facts show that this conclusion is well warranted. It stands to reason that the less the eliminative power of the kidneys is impaired, the greater should be the efficacy of a measure that spared the work of the kidneys. Altogether too little attention has been paid to an excessive protein diet as a potent cause in producing disease of the kidneys. Hearty eating, too little exercise, and high-pressure business cares are the factors that hurry many a valuable business man to his grave prematurely. If alcohol is added as a whip to keep up the pace, the progress of the disease is much accelerated. If these cases can be taken early, when high tension is first manifesting itself, the patients can be restored to a life of greater efficiency and relative health, and many a good citizen can be spared for an indefinite period of usefulness. To dietetic regulation in such cases must be added the lessening of the strain of business cares, moderation in all things, regularity of life, sufficient exercise and recreation, and thorough elimination from the alimentary canal. The treatment is essentially prophylactic. Drugs play a very minor part. When the profession awakes to the importance of the early detection of these diseased conditions of the circulation, and will have the patience to carefully study them and treat them thoroughly and intelligently, our statistics of deaths from these diseases will show great improvement as to the age attained, if not also in the number of ultimate fatalities.

DISCUSSION OF THE PAPERS ON CIRCULATORY DISORDERS.

C. H. Lawrence, Jr., M.D., Boston.

For the past six months I have been studying the effect of different preparations used for lowering the blood pressure. For this purpose cases were selected which had a systolic pressure of 180 millimeters or over. During the observations the patients were on a limited, definitely measured amount of liquids, and on a diet which did not vary from day to day. The amount of urine excreted was carefully measured and recorded. Observations were taken with the Faught sphygmomanometer, having an arm band of 12 millimeters in width. Observations were taken twice daily, at the same times each day, under all possible precautions to exclude the influence of meals, mental disturbance, etc. The drugs studied have been nitroglycerine, sodium nitrite and erythrol, mannitol, and a preparation called vasotonin. The effect of venisection, hot-air baths, high frequency and electric light baths was also studied. Systolic and diastolic pressure and the pulse rate were recorded at intervals of five to ten minutes, according to the drug studied, throughout the observation.

As Dr. Arnold has stated, I found that nitroglycerine had an effect lasting about ten minutes; I got no greater fall of pressure than 15 millimeters.

Sodium nitrite begins to act in ten to fifteen minutes, the maximum fall is reached in about twenty minutes and lasts twenty to thirty minutes, the original pressure being again reached in about two hours. The maximum fall obtained varies from ten to forty millimeters.

Erythrol produces about the same effect, except that the return of the original pressure is slower.

Mannitol gives about the same results as erythrol.

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With all these preparations it is essential to work with a fresh preparation, as old drugs have been found to be inert. This has been especially marked in the case of erythrol.

With vasotonin, a combination of Yohimbim and Urethan, I have had rather surprising results. Reports from Germany state that both in animals and in man it caused a fall of from twenty to forty millimeters, which lasted from four to six hours, and that three to four injections during as many days would maintain the blood pressure at a level lower than the original for about a week. In the five cases I have studied the results have been exactly opposite, the injection of the drug being always followed by a rise in pressure, and occasionally accompanied by more or less serious disturbances; one case showing a severe attack of angina pectoris, coincident with the rise in pressure. The duration of the rise averaged from two to four hours.

Venisection in the few cases I have had opportunity to study produces a gradual fall of from ten to forty millimeters, which is maintained for sixteen to twenty-four hours.

Hot-air baths, electric light baths, and high frequency are not constant in the effect produced, causing a fall of ten or fifteen millimeters in about half my observations. In an equal number no result was observed.

In all cases a fall in systolic pressure, caused by any of the above-mentioned methods, was accompanied by a similar, although less marked, fall in diastolic pressure, and usually by a temporary acceleration in the pulse rate. Respiration occasionally showed a temporary quickening.

In the eighteen cases I have studied with Dr. Edward Young the effect of a lowered pressure on the elimination through the kidneys. Our method was to perform the Geraghty Test at different levels of pressure, the amount of dye-stuff and of urine being carefully estimated at such levels.

In fifteen out of the eighteen cases more urine and more coloring matter was excreted at a lower pressure than at a higher one. The differences in some cases were small, but in all but three were outside the limit of error. In three cases the better excretion was obtained at the higher level of pressure. The tests at the different levels were performed as near each other as possible, in order to eliminate the

possibility of any change in the condition of the patient other than that produced by the change in blood pressure.

These results are rather surprising, and not in accord with the general clinical idea of the effect and purpose of high blood pressure. I have found in the literature a few articles reporting isolated cases in which the use of vasodilators was accompanied by increased excretion of urine, but no explanation for the occurrence has been offered, and I have none.

The fact, however, that the pressure may be lowered and thus lessen the work of the heart without lessening the excretion by the kidney, seems to be important.

SYMPOSIUM ON OUT-DOOR SCHOOLS AND OPEN-AIR ROOMS FOR CHILDREN.

Opening remarks by Dr. Arthur T. Cabot, Chairman.

The subject of our discussion today is one of the most important questions which confront the anti-tuberculosis forces. The proper care of the children during that time (the school age) during which the State assumes the responsibility for their right bringing up and for their protection while they are in the public charge.

The interests of education and the interests of public health meet in the study of this question.

The executive committee have invited, therefore, certain eminent students of educational matters to meet with the medical men of the Massachusetts Medical Society in this discussion.

SYMPOSIUM ON OUT-DOOR SCHOOLS AND OPEN-AIR ROOMS FOR CHILDREN.

ARTICLE XXXI.

OPEN-AIR SCHOOLS.

(*Notes of remarks made by* DAVID SNEDDEN, *Commissioner of Education of Massachusetts, at a Meeting of The Mass. Medical Society, June 13, 1911.*)

AMONG the numerous topics relating to better education, preventive medicine and social living which have recently claimed the attention of people with social interests, none seems to touch some of our pet hobbies and prejudices more intimately than that of Open-Air Schools. Already a considerable literature has been built up about the subject. Already, that comparatively modern institution—assisted ventilation—has received open condemnation. A new cult seems in process of arising. The usual number of enthusiasts, who see in this reform a panacea, has appeared. At bottom, however, there is much in the new movement that appeals to common sense, and that harmonizes with a growing conviction in education that, in dealing with children, one of the deadly temptations is always to get too far away from nature.

It is not the purpose of this paper to deal with Open-Air Schools, so called, as they relate to pathological cases. For our purposes, there are two classes of children, viz.—those who have become chronically invalid and for whom the recovery of health should be the controlling effort in any means undertaken; and the others, who are fairly normal as regards health, and for whom it is the aim of the

schools mainly to provide education, and, incidentally, to conserve normal physical well-being. It would appear to be doubtful educational wisdom to make education the central effort in dealing with the child who has incipient tuberculosis, persistent anæmia, or who is suffering from undernutrition. These cases should be relegated to the field of hospital practice—a phase of which might well be the Open-Air School for purposes of cure and the development of strength, and wherein the matter of replacing the home by the institution, in the provision of nurture and the like, can be taken up on a proper basis.

But the Open-Air movement, originating in pathological cases, seems destined to affect in important ways the educational surroundings of fairly normal children. It becomes a part of the great conservation program of the schools; it suggests ways and means of making school work more effective, without necessarily increasing the expense. The following considerations seem to be important in connection with Open-Air Schools as affecting normal children:—

I.—It should always be assumed that the demands of school life, like many of the other demands of civilization, impose artificial, excessive and illy-balanced requirements on the physical life of children. Through the ages during which processes of adaptation have made childhood and manhood what they are, children have grown up in an environment of sunshine; of exposure to the open air during the waking life; of a multiplicity of activities; of abundant opportunity for varied contact with the social and material conditions of life. The natural inheritance of childhood would seem to have made little provision for sitting in seats for four or five hours of the mid-day period; for breathing the impure and often dry air of a crowded room; for absence of exposure to wind and rain; for the diminution of exercise; for the persistent use of the eyes at

a focal length of twelve or fifteen inches; and for repression of speech during the time when the instincts of communication are most active. All of these subtractions from the scheme of natural life may be necessary to education and civilization, with modern educational administration. We must assume that each one of them exposes the child to the dangers of abnormal and imperfect development, and, as far as possible, their effects should be counteracted.

II.—An accumulating body of experience seems to prove that the prolonged exposure of tuberculous and otherwise unhealthy children to an abundance of fresh air with a humidity such as nature provides, and under proper conditions of protection as regards food, clothing and exposure to drafts, leads to the development of health and strength. If this be so, it seems to the layman that similar treatment applied to normal children would do much to prevent physical impairment of various sorts. It may prove impracticable to provide the various conditions of nurture, protective clothing and the like for Open-Air Schools for normal children, but it would seem to be possible to take some steps in this direction.

III.—It seems to be true that sensitiveness to cold, to bad air and the like are, to a certain extent, acquired habits. Experience seems to show that, where teachers and others have become habituated to a dry temperature of 75°, they feel unfit to live and work in rooms of lower temperature. In Massachusetts, it seems increasingly difficult to get children to walk some distance to school, and, when at school, to participate in playground activities, if the temperature is at all low. Whether the listlessness and lack of energy, which are said to characterize children studying in illy ventilated rooms, are matters of habit or of genuine physical depression, cannot always be certain. It would seem to be probable that the ages of selection, active since man was a cave-dweller, may have brought him to the point where,

during the night when dynamic activities are low, and, perhaps, the elimination of toxic products slight, he can live in unventilated rooms without loss, and possibly, with even a quieting of the nervous system. On the other hand, it is highly improbable that he has, more than in a slight degree, lived under conditions of low ventilation when bodily activities are strong and the elimination of waste products active. Hence, we may assume that vigorous intellectual activity, as well as physical, especially where a crowd is concerned, can only be carried on effectively under conditions of abundant ventilation.

IV.—While the present business of society is, to a certain extent, to apply curative measures to those found diseased, a vastly more effective function is to discover the origins of disease, and to close up the sources. Open-Air Schools for tuberculous children are important, but vastly more important is it to get back to the sources, and, by one means and another, to stop the manufacture of tuberculous cases.

V.—Probably the great lessons of the Open-Air School are to be worked out in connection with the teaching of fairly normal children. Artificial ventilation, even when produced at great expense, seems to have been a failure. It may be that we shall be able to provide conditions whereby in an ordinary school-room, during two-thirds or three-fourths of the school year, at least, conditions approximating those out of doors can be maintained.

The ideal school-room now, from the standpoint of light, is that which has practically one wall transformed into window space. We might imagine the upper half or two-thirds of this wall capable of being removed or adjusted so as to make possible a mild inflow and outflow of a gigantic current of air, so large and moving at such a rate as not to give the sensation of draft. We might be able to suggest to teachers and others who are abnormally sensitive to cold

air that more abundant exercise, dressing to suit the occasion, and a somewhat resolute determination to adapt themselves to circumstances would prevent the development of frequent colds among those who are so laden with the bacteria of influenza and other complaints as to be constantly exposed to nasal and throat troubles.

VI.—A comprehensive execution of the above plan might well involve the development of protective clothing for school children, to be used only within the school, itself. Teachers now know that among the poorer children, a very large number come to school with wet feet, and, even under conditions of warm ventilation, they suffer much and expose themselves to disease from the rapid radiation of heat from their wet feet. It is not impossible that school blanket suits and felt slippers might in time be substituted for the ordinary clothing, in order to make it possible for conditions of lower temperature and more abundant humidity to be maintained.

VII.—At least, the widespread interest in Open-Air Schools and open-air education compels us again to reorganize our notions as to the physical concomitants of the educational process. Let it be asserted again that we have a constant tendency to get away from a normal level in the use of the eyes, the breathing of air, the activities and postures of the body, and the exercise of the nervous mechanism. Again and again must educators be enjoined to study the physical conditions under which their artificial educational processes are carried on. It will prove possible in many respects yet unforeseen to achieve large educational results and, at the same time, to maintain in a greater or less degree a normal physical environment for growing childhood. It seems to be a law that growing childhood can only achieve its full maturity in an environment biologically normal. The adult is capable of withstanding far more adverse conditions, provided the initial processes of development have been realized.

SYMPOSIUM ON OUT-DOOR SCHOOLS AND OPEN-AIR ROOMS FOR CHILDREN.

ARTICLE XXXII.

OPEN-AIR ROOMS AND SCHOOLS.

By THOMAS F. LEEN, M.D.
OF BOSTON.

READ JUNE 13, 1911.

I HAVE been asked to read this paper as a physician and a member of the School Committee of Boston, but I wish it to be understood that I shall express only my personal views.

This subject is of the greatest importance to the public health authorities, for the future health and wealth of every community are dependent on sound, healthy bodies in our young, who in a generation will take our places in the world.

In 1894 the first school medical inspectors in the United States were appointed in Boston, and the importance of this initial step has been recognized, and the example followed by other cities in the United States. The influence of the work of the medical inspectors in the schools has so enlarged that it has amounted in a revolution in the care of school children. All those engaged in the work of preventive medicine recognize that the safeguarding of the health of our school children is the educational object of paramount importance.

Since the school at Charlottenburg, a suburb of Berlin, Germany, for anæmic or invalid children, was opened in the summer of 1904, workers in preventive medicine have real-

ized the possibilities of good to be derived by pupils being taught in the open air, having plenty of food, rest and mild exercise; in a school where education was subordinated to the effort to improve the child's physical condition, such education as was given being largely in manual training, drawing, singing, and nature study. Children under such conditions are not only benefited physically, but are mentally more able to assimilate the teaching given them. The Charlottenburg School and the German wood-schools are open only during the summer months, the children returning to their homes after school hours, a practice followed in the establishment of the English schools. None of these children were selected because they were tuberculous, but simply that they were anæmic or under-developed. In the schools that followed, in Providence, Boston, and elsewhere in the United States, anæmic children, as such, were not accepted, the attempt being made to admit only those who were actually tuberculous.

There are some fundamentals concerning the care of school children which I think we all ought to understand and agree on, before we attempt to direct public expenditure in this matter.

We all agree that the greatest waste of to-day is the waste of human life, and its conservation should be the chief aim of public hygiene and preventive medicine. At present, little real progress has been made in the effort to rid the community of diseases known to be preventable; and if these methods are ever to be successful, we must begin at the bottom—in the schools.

Free education is given by the State to develop a citizenhood that will be primarily intelligent, able to do its full civic duty, and fit properly in the economic life of the community. Almost up to the present our purpose of education has been particularly to the training of the mind, but daily it is more apparent that the neglect of the body, and failure

to incorporate into our educational system hygienic instruction and cure, carried to its logical end—the prevention of diseases of childhood and the preparation and turning out of a boy or girl who is able to do its full share in life's activities—are the most important objects to which we must direct our attention. If the school system turns out a child at fourteen with a weak body, the money spent in education on this child generally has been wasted; this does not take into account the enormous loss to the parents and community, spent on its upbringing to this point. Therefore, all our efforts in school hygiene must be so focused that the educational investment, which is in fact an insurance for good and efficient citizenship, shall be protected.

It is agreed that up to 75 per cent. of all human beings, at some time or other, have the tubercle bacillus, but death results only in not more than 10 per cent. From the time that the child goes to school, at five years of age, until the end of the elementary school period, at fourteen, there are comparatively few deaths from tuberculosis, although it is believed that 50 per cent. of all the children in the public schools may be carriers of the tubercle bacillus, which, though dormant during the elementary school period, manifests itself when they are obliged to go to work. The result of the pressure to earn a living under present day conditions, in factory or store, handicapped by such defects as lack of teeth, poor eyesight, defective hearing or pharyngeal and bone troubles, common among school children, and hitherto practically neglected by the educational authorities, is shown in the enormous death-rate from tuberculosis between fifteen and thirty years, when over 35 out of every 100 of all the persons who die succumb to this disease.

There is no disagreement regarding the value of open-air schools for anæmic children and those who need watching to prevent disease. The only question we have to consider is how far this open-air education should go. This leads

us directly to the consideration of how we are to attack this problem in order properly to achieve successful results.

I have attempted to get from the medical school inspectors an accurate statement as to the number of anæmic children under their charge, and of those suffering from pulmonary and other forms of tuberculosis. The figures as to the number of children needing out-door schools and hospital care are a surprisingly small percentage of the total number of children in the schools. In view of the lack of a more complete medical examination, it would be unwise to draw any definite conclusions from them. The reports indicate, however, that tuberculous children of all kinds do not constitute more than one-half of 1 per cent. and the anæmic children about 2 per cent. of the total number.

In my opinion, the best way to meet this question is the strengthening of the medical division of the Educational Department. To accomplish the best results, the child from the time it enters school life until it leaves it, should be under the complete control of the School Department, which should aim not only to fill the child's mind with knowledge, but its body with health. During the formative period of childhood, which we may consider the first fifteen years of life, the growing child has essentially different needs and desires from those of the adult, and must be always separately considered and have special provision. Action by the State to aid the child's growth and development, physical and mental, from the time it ceases to be entirely dependent upon its parents at five or thereabouts, until it reaches the age of fifteen years, must be considered as constituting its formal education.

During the education and physical up-bringing of the child, any attempt to divide duties or functions between the school committee and any other city board is bound to result in increased cost, making for duplication, and will most likely result in actual failure to accomplish results.

The Medical Division of the Educational Department, therefore, under whatever name it exists, is one of the most important branches of school administration, and its duties, from the time the child enters the kindergarten until it graduates, should be directed primarily to keeping that child well, to studying the defects, mental and physical, which tend to nullify or to limit the educational advantages offered to it, rather than waiting until the child becomes diseased for any reason, and then relieving its distress. A complete record of the physical development of the child should be kept year by year.

To put these preventive measures into force and apply them to all the children, it is absolutely necessary to enlist the co-operation of the public. The educational authority, through its medical division, should detect at the earliest possible moment every deviation from the normal growth of the children under their charge, with its cause and extent, and having found the diseases in childhood, the remedies which are necessary to relieve or remove should be applied. This systematic and intelligent research to discover the causes for these defects should be sought in the home and elsewhere. The medical inspection in the schools is not efficient, when it only aims to detect these children suffering from those diseases which, if the children were allowed to remain in school, may be communicated to others. Defects of whatever kind, brain or body, which after that child is turned out of the school are liable to make it the prey of tuberculosis or any other kindred trouble, should be searched for, and remedied, and our school medical system will not be efficient until this is done.

It is not the function of the educational department to run hospitals which should come under the control of a separate department, or to relieve parents of their responsibility to provide food and clothing for their offspring; and most particularly, in all matters connected with the prevention

of diseases in the public schools and the turning out of a mass of children with healthy minds and healthy bodies, the child must be protected from even the semblance of the stigma of charity. The duty given to parents by God, to take care of their own children, should never be in any degree taken from them by the School Committee or any other public authority. The community which attempts this would be entering the home and breaking up the family, which is the foundation of society.

The organization of the School Medical Department under the School Committtee will bring these various problems into correct relation; it will result in financial economy. When transient conditions of ill health force the child temporarily to leave the schools, it should come under the control of the sanitary authorities, and this is now agreed by scientists to be the proper line of legislation in these matters.

The Directory of the National Association for the Study and Prevention of Tuberculosis, just issued, shows that while open-air schools and classes for children have been established, or are in contemplation of establishment, in fourteen states, no definite line of action has been agreed upon.* Each state seems to be working independently in the effort to solve a problem without giving any consideration to the question of finding out what that problem really is. Practically all the schools now established are open-windowed rooms or day camps, and are for the care of anæmic children or those predisposed to tuberculosis.

As I have shown, not even in Boston, which led in the work of employing school physicians, can there be found definite statistics which can be accepted. Accurate statistics, so that we may determine our problem, should be the object of our first and most united effort. At this point, I wish

* Tuberculosis Directory. National Association for the Study and Prevention of Tuberculosis, 1911. Open-air Schools and Classes for Children in the United States. Page 131.

to say that I consider the staff of school physicians of the Boston Board of Health as efficient and competent as any body of doctors who might be chosen by Civil Service or otherwise for this duty. The sweeping criticism of these physicians which has been made is, in my opinion, most unjust, because whatever fault is to be found with the results of their medical work in the schools, should be attributed not to the doctors on the staff, but to the system under which they work; and until this system is radically changed, and the school physicians are wholly subordinated to the School Department, conditions are not likely greatly to improve.

The present situation, where the Board of Health has control of the school physicians, and the Directors of Hygiene of the school nurses, is absurd. The school physicians should be under the direct control and supervision of the educational authorities and subject to the orders of the Director of School Hygiene. When it becomes necessary for any reason to remove a child from the school to give it hospital care, that child should pass out of the care of the school department into the special department treating such disease. Whether the child has diphtheria, scarlet fever, measles or tuberculosis, the principle is the same.

It seems to me, therefore, that the school committee should go ahead to make its preparations for out-door rooms as planned. In addition, all the children in the school should be safeguarded from existing defects of ventilation and other bad hygienic conditions. Children anæmic, mentally backward, or in other ways not receiving the full benefit of their education, should be brought together in separate classes in the open-air, always as far as possible in the localities in which they live. The wholesale extension, however, of this out-door school idea, should proceed along conservative lines, until the result of a thorough medical inspection shows in detail the character of the task

before us and the amount of labor and expense it involves. If, for instance, as a result of this medical inspection there are found 200 children with demonstrable tuberculosis in a stage of the disease where they will be likely to transmit it to their fellow pupils, they should go to a hospital provided for such cases, administered by the department created for that purpose. When we find the number of children with demonstrable tuberculosis not dangerous to their fellows, it will then be time to plan for rooms for these special cases, but similar provision should also be made for the mentally defective, the epileptics and others when they are found. The problem of the out-door rooms for the anæmic children is a simple one. In all these matters, however, we should not go too fast.

The important point, in fact the only point in dispute, regarding the question of the open-air room is that of the free feeding of the children accommodated therein. Should children who have diseases, or who are likely to have them, which can be prevented by proper food, be fed at the expense of the tax-payer or at the expense of the parents? Everybody who visits schools may see many children coming from the districts in which the poor live, who apparently have the signs of hunger written on their faces. The first and natural assumption is that such children are actually suffering from want of food, and that it is due to the inability of their parents to provide it. From this premise it is easy to jump to the conclusion that the child who looks hungry is actually hungry, and that if it is hungry it is always because the parent is unable to provide food for it. The danger of these assumptions is that if we go to the districts in which only the comparatively well-to-do reside, we find there also pale and sickly looking children who, if they were sent to school, poorly clothed and with unwashed faces, would excite the same sympathy as the poor.

The reason for the physical condition of anæmic children

is most frequently sought elsewhere than lack of food and inability to furnish it at home. While there are unfortunately some poor who are unable to buy proper or sufficient food for their offspring, speaking generally, poverty of this degree is rare in this city. Those who know the honest poor, also know that the last thing that they will do is to endure the suffering of their children, and to this end they will sacrifice their most sacred possessions in order that their children shall not suffer hunger. Inquiry among the school nurses will show that the children who are apparently starving are the ones who are also frequenting the moving picture shows, and other places of cheap amusement.

Due to lack of accurate data upon which to base exact conclusions, we can only say at this time that it is probably true that if children are sent hungry to school, except in the comparatively few cases of the honest and poor parents for whom the city is ready and glad to make suitable provision, these are the children of the lazy, the drunken and the careless; or those who, with an imperfectly developed sense of parental responsibility, are willing to shuffle off the duty of providing for their children if they think the city will undertake it for them.

These matters have all been thrashed out in the British Parliament, and references to debates in the House of Commons on this matter throw a flood of light on this question.*

To sum up, the first and most important thing is to get the exact knowledge of the physical condition of the school children. This can best be accomplished by the making of the Department of School Hygiene under the school committee, what it really should be, a real medical inspection department with full charge of the school medical inspec-

* Edinburgh Review, October, 1906. "Socialism in the House of Commons." Hansard's Parliamentary Debates. Session, 1906.

tors, thus giving the school committee complete charge of the children under their care.

Next, the establishment of open-air rooms for anæmic children in which rooms, if food is to be given, it shall be supplied by the parents, or in cases of necessity by private charity.

The turning over to the proper authorities for hospital care all children, association with whom would be dangerous to the other pupils.

We should more generally open up the windows and roofs to all the children in the public schools, and when children are found who are not getting proper food at home, and whose parents say that they cannot provide the school luncheons as required by the schools, careful examination should always be made to ascertain why such parents are really unable to provide the food, and the reason therefor; so that in case of need it may be provided by private charity.

SYMPOSIUM ON OUT-DOOR SCHOOLS AND OPEN-AIR ROOMS FOR CHILDREN.

ARTICLE XXXIII.

REPORT ON REPLIES TO A CIRCULAR LETTER

Concerning Fresh Air Rooms and Outdoor Schools, sent out by the Associated Tuberculosis Committees of the Massachusetts Medical Society.

By JOHN B. HAWES, 2d, M.D.
of Boston.

READ JUNE 13, 1911.

On April 10th, 1911, at the suggestion of the Suffolk District sub-committee, the Associated Committees of the Massachusetts Medical Society for the Prevention and Control of Tuberculosis sent out a circular letter in which certain questions were asked concerning the subject of open-air rooms and hospital schools. A copy of this letter was sent to: 1. Each member of the Associated Committees; 2. The School Superintendents of this State; 3. The Boston school physicians; 4. Certain other persons, not physicians, known to be interested in this subject. As the opinion of the majority of the Board of Trustees of the Boston Consumptives Hospital was already known, copies were not sent to the trustees as such, but only to such members who were already included in one of the above mentioned lists.

The object of this inquiry was to secure if possible a broad consensus of opinion on certain phases of a most important and somewhat complex problem. The questions asked concerned two distinct problems—one, that of fresh

air rooms in or connected with school buildings, to provide for certain children who were simply anemic or run down, but not necessarily tuberculous (Class A.); and two, that of hospital schools, separate institutions connected with or under the auspices of proper hospital authorities, where children who are manifestly tuberculous, and who in the ordinary public schools would be a menace to the health of the other children, could be cared for as regards their health and in addition be given such schooling as they are capable of undertaking. These latter children constitute Class B.

In answer to question 1 — "Do you believe in the general principle of open-air rooms and open-air schools?" — out of about 200 replies, 196 answered "yes," 4 "no." One person believed in fresh air rooms, not in hospital schools, and two persons believed in hospital schools but not in open-air rooms.

Question 2 — "Do you believe that children in Class A attending a fresh air room in a school should be provided with lunches?" — met with a nearly equally decided answer. 179 answered yes, 15 no, to this question.

The answers to question 3 — "Who shall pay for these lunches in case the child's parents are unable to do so?" — showed a wide difference of opinion. In answer to this, 110 said that the city should provide such lunches, 20 the school department, and 22 were of the opinion that such lunches should be provided by private agencies. The opinion of those most qualified to decide as to this question seemed to be that except in extraordinary cases the parents of children attending fresh air rooms in a school could pay the small sum necessary for a lunch—one or two cents—and that in the few cases in which after careful investigation it was found to involve hardship or that it was impossible for the parent to provide even this small sum that private agencies should undertake this task. One eminent gentleman made

the statement that "if such lunches are to be furnished from the taxed there are many other things which should also be furnished from the taxed," while another person stated that extra clothing and food should be provided at public expense, like text books, but only after careful investigation.

Question 4 read as follows: "Granted that the children in Class B, attending a hospital school, should be provided with food and the necessary extra clothing to keep them warm, who should provide this food and extra clothing in case the child's parents are unable to do so? One hundred and forty-three were of the opinion that the city or one of its departments, the board of health or the hospital department, should provide such food and clothing; 22 believed that private agencies should do this, and 11 that the school department should bear this expense. The general opinion seemed to be that in case the parents were unable to furnish or pay for the child's food and extra clothing it should be provided by the hospital authorities as is done in the case of any patient attending any one of our large general hospitals or state sanatoria.

Question 5 — "Should the open-air or fresh air rooms be under the control of the school department or a medical department?" — brought out a large majority of answers, 139 being in favor of such fresh air rooms being under the control of the school department or its physicians, while 55 believed that such rooms should be under the supervision of a medical department such as a board of health.

Question 6, asking the same question concerning the hospital schools — "Should the hospital schools be under the supervision of a school department or of a medical department?" — was answered in the opposite way. A large majority—120, believed that the hospital schools should be under the direct supervision of a medical department, 38 were in favor of the school department, while 17 believed that both departments acting jointly should control such a

school. Many persons living outside of Boston in smaller cities and towns were emphatic in their belief that although such a hospital school should by all means be under medical supervision it should not be under the local board of health. One reply was as follows: "Local boards of health have so far failed to meet their opportunities. The reason why I place such schools under the care of school committees is because they are out of politics. The average board of health is ill paid, has a multiplicity of duties, and does few of them efficiently." Another gentleman writes: "There is usually more politics in boards of health than school boards. In my city I should prefer the school board to be in control." There were numerous other replies in the same vein.

Question 7 — "What would you consider the best means of selecting children who should go to such schools, the school teachers, school nurses, school physicians, or physicians of a special department, such as the Boston Consumptives Hospital Out-Patient Department?"—brought a very large majority of replies in favor of the school physicians and school nurses as the proper persons to select the children who should go to the hospital school. One hundred and eighteen answered in this way: 40 believed that doctors of a special department should make such selection, while a few were of the opinion that all agencies should act together. It seemed to be the general opinion that in a great majority of instances the school physicians and school nurses would be perfectly able to make such selection, and that where there was any doubt the opinion of physicians of a special department could wisely be asked.

Question 8 — "What suggestions or plans would you make for establishing a system of open-air rooms and outdoor schools for Boston?" brought a multiplicity of interesting and valuable replies, covering too much ground

and involving too much time to be considered in this brief report.

For the purpose of carefully considering the results of this inquiry as well as the papers presented and the opinions expressed at this present meeting, and to formulate from this material a statement covering the general situation of fresh air rooms and hospital schools, which will be of value to the numerous smaller cities and towns in this state, which are already considering or will shortly be considering this question, I would suggest that a committee be appointed to undertake this work and to embody the results of their efforts in writing. I would formally present this suggestion in the form of a motion to be acted upon at this meeting.

DISCUSSION OF THE PAPERS ON OUT-DOOR SCHOOLS AND OPEN-AIR ROOMS FOR CHILDREN.

DR. ALFRED WORCESTER, Waltham: I have been greatly interested in the subject which has come before us this afternoon. It is a most gratifying thing that so large a company gathers to discuss this question. It is the first time that it has been discussed, so far as I know, by any medical body; it is also very fortunate that there are such diametrically opposed opinions. It is an evidence of awakening interest. In so many of our communities where at first there is enthusiasm, that enthusiasm soon fades away. Everything is applauded that anybody suggests, and all sorts of experiments are proposed which would seem to make a good campaign, and then there is a general apathy. If there is difference of opinion at the first meeting, there may be some hope that the movement will continue. It often happens that all are agreed about this thing or that thing—of course we must do it—but when the question comes as to who shall do it nobody comes forward and the movement drops. Now, for the first time in any part of this campaign we find a difference of opinion, and I am sure you will agree with me that the greater that difference is the larger the promise is that we shall do something.

I want to make this point; that the environment of the child in the schoolhouse may be different from an ideal environment in the home. I do not think we have gone too far in believing crowded school rooms and dwelling houses to be a causative agent of tuberculosis, although the Eskimos crowd together and breathe in worse air than any other people, and live in filth, and yet are free from tuberculosis.

I think we ought to look at it in this light. Nearly everybody has tuberculosis, but only those succumb whose vitality is low. Anything that we do that increases the vitality of the child will tend to prevent that child from

succumbing to tuberculosis. We know that fresh air and outdoor life will do it, so why should we not provide it? I do not believe in the idea of pauperizing poor families by giving a lunch to a child that needs it. Why not give to all children? Boys at West Point are fed and clothed there at the public expense. Of course the country is justified in providing it, because we need them in the future perhaps to lead the army. But we also need the privates who shall serve under their command. I do not think it is a question of temperature. I think that a great deal of the difference of opinion is due to the confusion that exists in the medical profession between preventive and curative treatment. It is only a little while since the idea of physicians having any preventive function came to notice. Latterly it has seemed perhaps as if these preventive and curative functions were of equal importance. The preventive function is far more important than the curative function, and just as soon as that idea obtains possession of the people, just as soon as we can make that fact established, then you will not find so much confusion regarding our duty in this matter as physicians, and the question as to what shall be done with the children.

When the time comes that our living depends upon keeping the children well, then the medical profession will be united in having out-door schools and feeding the children up to the full limit every single time.

Dr. G. S. C. Badger, Boston: In discussing this question of open-air rooms I find that a good many are in doubt as to just what it means. The question of tuberculosis is mixed up with the question of the physical welfare of all school children. The open-air school room is not wholly a question dealing with tuberculosis in school children. Open-air school rooms are going to be a benefit to a great many school children who will never have active tuberculosis. They are all going to have less disease and a greater enjoyment out of life.

I do not believe that there is much active tuberculosis in the public schools, and if I may I will briefly call your attention to some of the statistics. Under 5 years of age, in the registration area of the United States, there are

approximately 1500 deaths from pulmonary tuberculosis. Between the ages of 5 and 10 there are 1900. Between 10 and 15 there are 1500. Between 15 and 20 there are 5000, and between 20 and 25 there are 14,000. What has the public school system to do with that tremendous number of deaths from tuberculosis? Is the public school life responsible for the breaking out of such an enormous amount of tuberculosis? I do not believe it is. I think it has to do simply with the change of life and environment and the stress that goes with earning one's living.

In the public schools the essential thing seems to be fresh air. In practically all the meetings greatest emphasis is laid upon fresh air, and the open-air rooms established in the Boston schools have done good not only in the four buildings where it has been tried, but every other room in these buildings has been better ventilated because of them, and it causes improvement in the physical condition of the children in all the rooms. If every city and town would insist upon having open-air rooms, there would be a great benefit in the health of the school children. I do not believe that the modern ventilating of buildings works a success. The air is vitiated; the temperature is rarely what it ought to be; the air is excessively dry. There must be open air coming from outside, without any contamination by modern apparatus.

One thing that has been left out, and that is, whose opinions are to have weight in deciding these questions? In the physical welfare, who is going to sum up how well the child is? What tests can be made to tell that the child is physically unable to do his work well? I do not think there is any test in the majority of cases, but the teacher can tell you about it, and she is the one who can give very valuable information, and in the Boston Schools she has done so.

I think that the nurses and the medical inspectors should be under the school authorities, but when a child has tuberculosis or any other disease dangerous to the public, the child should go out of the school, and go under the care of the proper hospital authorities.

Now, the open-air school rooms and the hospital schools, I think, are going to have a changing population. The

children that are run down and physically below par—anemic, if you want to call them that—will stay in the open-air rooms until well enough to return to the regular class. The children who leave the hospital will go back to the open-air room. In the course of the year, as different pupils become anemic or debilitated, they will be sent to the open-air room for the time necessary for a return to good health. If this improvement in health in the open-air rooms for the debilitated is so marked, is it not a strong argument for the making of all rooms more open to the fresh air?

Dr. Benjamin T. Loring, Boston School Physician: It is an indisputable fact that a considerable percentage of school children are anemic, have enlarged cervical glands, are undersized and pale, and having diminished resistance to disease, become easy prey to various infections. They are frequently absent on account of illness, and their minds and bodies suffer to an extent which handicaps them for the remainder of their lives.

The open-air school has lately become a factor in the endeavor to remove the underlying causes of these manifestations of poor health, and deserves and will receive vigorous support from our profession.

We are glad for this sign of progress, and thankful for the growth and support of intelligent public sentiment. But let us not deceive ourselves into thinking that we have done our whole duty by the delicate and unfortunate children when we have put them into an open-air schoolroom.

Observation of such children in two average districts of this city long ago brought me the conviction, which has steadily grown stronger, that while fresh air will help, the real problem is one of nutrition. Any effort which supplies fresh air alone where nourishment is needed, can only meet with a small measure of success.

This is the one point which I hope to emphasize by this discussion, and which I believe to be of paramount importance.

On the records of scholars selected to be in an open-air room now being organized, the school nurse wrote what she knew regarding the home conditions of these scholars, who had been selected without suggestion or help on her part. Here is her report on ten consecutive cases:

Child 1. No father. Brother under treatment at Burroughs Place. Family receives outside aid. Gets little fresh air at home.

Child 2. Home poor. Seven children. Out of school most of 1909.

Child 3. Mother slack. Many children.

Child 4. Home poor. Both parents work. Father intemperate. Child is much alone and gets her own meals.

Child 5. Drinks much coffee. Home conditions fair. Very little fresh air at night.

Child 6. Home fair. Mother careless. Step-father recently returned from camp treatment for tuberculosis.

Child 7. Family frequently has help. Parents both work. Father drinks. Seven children. Small income.

Child 8. Excellent home. Under doctor's care. Sleeps out of doors.

Child 9. Home fair. Step-mother does not take much interest in child. Has had a bad cough for two years.

Child 10. Father and mother both work. Gets his own meals.

These are cases just as they run, and are not selected except as being those selected to go in an open-air room. Here among ten children are two, aged 11 and 12 respectively, one boy and one girl, who get their own meals. The boy has enlarged cervical glands. The girl has hypertrophied tonsils: has had pneumonia once, and has frequent attacks of bronchitis. One only of these ten had a good home and care. He is pale and flabby, and is convalescent from a long attack of enteritis supposed to have been of a tuberculous origin.

Of the three whose homes are fair, one has a careless mother, and a step-father returned from a tuberculosis camp, one has a cough of two years' standing and enlarged cervical glands, and one gets little fresh air at night and drinks coffee freely. He is undersized and has enlarged cervical glands. The remaining six from poor homes are distinctly cases of insufficient nourishment due to large families and small incomes, and in two instances to intemperance on the part of the father. Three of the six have enlarged cervical glands, two have hypertrophied tonsils with frequent attacks of bronchitis, and one is a plain case of ill nourishment.

I have seen the records of the scholars in an adjoining but poorer district than that from which these are taken, and there is twice as large a proportion of similar cases (6 % compared with 3 %), so it is certain that the conditions reported are not exceptional.

It can readily be seen that parental incompetence and home conditions other than the lack of fresh air underlie the bulk of these cases. To such children what advantage is a better appetite and increased metabolism? It is exactly like opening the draft in a stove and paying no attention to the supply of fuel.

This plain but difficult problem of our School Committees is not only hygienic, requiring fresh air, but is to even a greater extent sociological, requiring nourishment and in some cases clothing.

Only as these requirements are met have we reason to expect the good results which follow when we "remove the cause," thereby appoximating one of the ideals of modern medicine, namely the prevention of disease.

MR. ADELBERT L. SAFFORD, Supt. of Schools, Chelsea: The general principle of fresh-air schools for tuberculous and anemic children already seems to be rather generally accepted in theory. The possibility is also recognized that fresh-air schools may prove very invigorating and salutary for children in ordinary health.

Beyond this point, most school and health officers, I imagine, have very few definite notions.

Published plans for carrying on such schools are mostly vague, experimental, make-shift or appropriate to particular conditions only. Important details essential to general application are lacking.

There is also an absence of authoritative and convincing demonstrations of the precise agencies beneficial to health in such schools. Shall we attribute the beneficial qualities to the fresh air wholly rather than to feeding, rest, play, and the elimination of toxins? If the fresh air is the essential element, is this due to the oxygen in the air, the temperature of the air, the humidity of the air, the absence of noxious gases, the free movement of air currents, or to some hitherto unrecognized property or condition of the air?

Several writers seem to believe that the beneficial principle is some little-understood property of the open air and speak of radiant energy, ionized air, sunshine and ultra-violet rays.

We should all like, I think, some more definite assurances on all such points.

There are many details of feeding that are in great need of illumination.

Then there are the questions of the precise function, organization and source of control of the different types of schools, such as (*a*) fresh-air schools for children who have been definitely pronounced tuberculous and should be excluded from all association with healthy children in the regular schools. Such schools must of necessity be separate institutions and might perhaps be adjuncts of hospitals; (*b*) fresh-air schools for children with tuberculous and anemic tendencies. These schools may be in fresh-air rooms connected with regular public schools; (*c*) fresh-air schools for normal children. What constitutes a suitable fresh air schoolroom? Should it be open on all sides or on one side only? Is an ordinary schoolroom with the windows removed sufficient? Cold air is not necessarily fresh air. Will an ordinary schoolroom with the windows removed be sufficiently ventilated? Some authorities contend that it will not.

There is even great danger in the enthusiasm of the "fresh air" advocates among the teachers who recklessly throw open windows and expose pupils without proper protection to draughts and cold.

Then there are the questions of heat, wraps, sanitation and program. In short, before open-air schools can be generally established with success, there must be a more nearly standardized practice in their management. Expert investigators and scientists must carefully go over the whole ground and present a more authoritative and detailed plan.

MR. JOHN L. RILEY, Superintendent of Schools, Holyoke: Five minutes is a pretty short time in which to say anything on such a big subject.

In one of the European countries they say that the air of the towns is pure because the peasants never open their

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windows. The experience of school teachers would lead them to say that the same might apply to some of our cities.

I was very much pleased with the two addresses that were made this afternoon. I was glad to hear Mr. Snedden emphasize so strongly the importance of the subject of ventilation in our school buildings. I was pleased with Dr. Leen's reference to the need of medical supervision being perfected and made more efficient through the school authorities having a larger control over it. Under the present conditions, the intent of the law, as I understand it, is that every child shall be submitted to a thorough and careful and separate examination, in order to discover any tendencies or conditions which may exist that would tend to make a child unfit to receive such education as he is entitled to receive.

As a practical school man, it seems to me that the danger to the health of children is very much greater in our elementary schools than in a high school, and in the middle and lower grades in the elementary schools than in the upper grades. The classes in a high school are smaller, and they stay in one room only one period. A teacher who regulates the temperature of the room by her own body, rather than the thermometer, is quite common. Then one child is obliged to remain in one room with the same teacher over five hours. In the high school and upper grammar grades students go from one room to another, but in the primary and middle grammar grades a child must sit under the same conditions for five hours (possibly five and a half hours, if he whispers). I know the conditions well enough to realize fully that our schools are not entirely responsible for the unfit conditions of children, but I do believe, after having lived right in the class room myself for years, and after having felt the effect on my own health of what someone has called "baked air," for five or six hours a day, that the question of temperature is a very important one in the health of our children in the elementary school.

Of the three types of schools, the out-door school I consider the one giving much more promise than the other two. In the Providence open-air school they use large size windows, attach them at the top by means of hinges, and raise

those windows against the ceiling and open to the fresh air. That is the famous school at Providence. There was also a school at Franklin Field, Boston claiming to have had the first out-door school.

Holyoke has recently opened a new building which embodies an idea that I do not know has been put into a school building before. We used corner rooms, south and east, as sun rooms. We have used five large windows, eight feet high and three feet wide, pivoted top and bottom so that they can be thrown wide open. I am not ready to say that we shall put them in all our new school buildings, yet we feel confident enough to have the subject under discussion now.

It seems to me that this method of ventilating several rooms in every school building by some such means as pivoted windows has great promise.

If I might suggest it to this body of men, we superintendents are eager for information as to how our school buildings ought to be ventilated. The school board in Holyoke has now in contemplation the erection of a fourteen room building. They are asking for suggestions; eager for them. I feel dissatisfied with our system of ventilation. The janitor simply tries to make the building warm enough. I firmly believe in the thermostat system of regulating the temperature. If your association could make some thorough investigation into the condition of ventilation in the school buildings and evolve some schemes whereby the ventilation could be improved, I am very sure you would be thanked for it.

In connection with the medical inspection, I feel equally certain that if this body would send out to every physician in Massachusetts a little circular asking his aid in supporting medical inspection, or making it more perfect,—if your body would do that, it would help the cause of that inspection a great deal. I can only say that we superintendents are eager to note the issues and to have some intelligent and reliable authorities point out the remedies, and we are ready and willing to experiment, to the end that the health of these children may be better conserved, and that the homes themselves may be purified through the knowledge that we send out by means of those children.

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DR. A. K. STONE, Boston: I have recently been on a small committee representing the Boston Anti-Tuberculosis Association and the Boston Society of Architects, who are considering this question of ventilation. I find that this is very much discussed among the architects. They are very much dissatisfied with the present conditions. They are opposed to the elaborate and expensive system that has been adopted. What they are going to recommend is an entirely different matter.

I think that they are eager to work on this very great problem, and they will be able to report upon it in a comparatively short time. They are collecting a large amount of data and trying to get some definite statement that they can make. At present they are inclined to believe that fresh air, as has been said here today, directly admitted to the room, is very much preferable to an elaborate system of ventilation.

I have here some plans that have been drawn for the making over of the school buildings in Boston, and also another plan proposed by Mr. J. R. Coolidge for a school where 1000 children may be accommodated and practically all be out of doors, and yet have sufficient heat to do away with some of the ordinary heating facilities.

MR. FRANK MANN, Secretary of the Tuberculosis Committee of the Charity Organization Society of New York City: I came here this afternoon looking for information. We have been responsible for the starting of the anemic classes of the public schools of New York. We have five at present, and two more to be prepared during the summer. We also have the assurance of about twenty more during the coming year. At present the committee on prevention of tuberculosis is feeding the children in the anemic classes. It is costing at the rate of fifteen cents per child per day to feed these children. We give them milk in the morning, as much as they want; a hot meal in the middle of the day, consisting of soup, bread and a dessert, and sometimes cocoa; and we give them milk and bread again in the afternoon, as much as they want. The food that we give them costs a little more than eight cents per child per day, and in addition it costs six cents per day per child to serve it to them.

If it is true that 2% of the children in the public schools are anemic, we would require in New York about 700 classes of twenty-five children in each class, and at the present rate that it is costing us it would take $500,000 per year to feed them, and I can speak for the tuberculosis committee that they won't do it. I do not know who is going to do it.

I would like to ask two questions. First, what is being done for the children in Boston as to the feeding? Second, what provision is being made for the children in the proposed classes?

I do not know just what we are going to do in New York in another year, because it is very evident that with the heaping up of expenses, private charities have got to get from under the load. I know that some of the executive officers in the board want the food in the schools; I know also that some do not want it. What is being done in Boston now?

Mr. Charles J. Jenkins, Jr., Brooklyn, N. Y., Secretary of the Committee on Tuberculosis of the Brooklyn Bureau of Charities: There has been a great deal of valuable information given here today. I would like to go one step further, however, and say that in the schools in New York, where they feed the largest number, Dr. Wild says that three cents covers the expense of the lunch, and of course somebody simply has to pay the administration expenses. The children in this school all pay for their lunch. If they cannot pay three cents for a lunch, the family is looked up by the organized charity organization.

You have all admitted that fresh air, that is, air from outside, is better than artificial ventilation. Why should not all the children be provided with good, healthy lunches in the public schools, and why should we not have fresh air in all the schools and all the rooms?

Dr. T. F. Harrington, Director of School Hygiene, Boston Public Schools: For the past two or three years a great number of experiments have been tried in Boston. The data of something over 5000 children was classified. Our first problem was to find out if possible the home con-

ditions of all these children. We had our corps of nurses who knew pretty well all home conditions, and our returns show that 70% of the so-called anemic children came from good homes, and the other 30% from poor homes. Over 65% of the children coming without breakfasts came from homes where breakfasts could have been provided. Lack of food for children is not always due to poverty in the home. We have come to the conclusion that it is not a question of poverty.

We tried many experiments, based principally and wholly on that subject, and concluded that the children should pay for the food provided. Teachers have collected from the children a penny a day. The teachers have been able to provide enough milk and jam and bread and butter to give the children a substantial meal at mid day. Our condition is different from New York in this respect—we do not have to furnish a mid-day meal. Our school children go home or leave the building during the noon hour. However, the children are provided with sufficient food.

Results have led us to the recommendation that in the establishment of open-air rooms and open-air schools, facilities should be made for permitting the children to get an opportunity to secure a lunch at a cost not exceeding two cents in the middle of the forenoon.

DR. J. F. TWOMBLY, Brookline: In one school in Chicago, there are twenty-eight rooms altogether. It is a graded school. Fourteen of these rooms now are fresh-air rooms—one half of the school. They began first with the kindergarten, and in that school it has been found that the children in the fresh air rooms are in much better condition, from the point of view of the teacher. They can study better, the discipline is much better. There has not a single child complained of the fresh air. In fact, every teacher wants to be transferred to the fresh-air rooms. The windows there are kept open as much as possible, on both sides of the school when possible; when there is too much wind or rain, they close them on one side. The average temperature has been about 50°. The heating is from the walls and the halls, but the heat is turned off from the schoolrooms themselves. The windows are kept open,

and the children are warm and so are the teachers. There is no special feeding in that school, but every hour they can romp around for five or ten minutes. No special clothing is provided, but some of the children put on sweaters or overcoats if they want to.

From the point of view of medical examination, four rooms of the first and second grades were taken for comparison. Two rooms were ordinary rooms with a supposedly good ventilation. The other two rooms were fresh-air rooms. In the latter the doctor found 2% suffering from colds. There were 40% in the other rooms. That may have been a mere accident, but that is what occurred.

Dr. H. L. Dearing, Braintree: I should like to say one word regarding school teachers. I come from Braintree, where I have been interested in this work for several years. I visited one large building and interviewed the teachers. I found that the younger teachers were having plenty of fresh air while the older teachers had windows completely closed; conditions were not fit for anyone to remain for any time in the room. This shows the necessity of co-operation between the educational and the health sides of the question, and I wish that more superintendents of the schools could be with us at such meetings as this, that they might get a definite and a more far-sighted view of the situation.

In Braintree we have not been obliged to form an anti-tuberculosis society, because we have a friendly aid society which has done a great deal in furnishing food for our children and helping educate the teachers. It is absolutely necessary in my experience to get at the teachers.

I simply wish to say further that I hope we will all consider ourselves a committee of one to open our mouths a little wider than we have in the past and to get at the teachers.

Mr. Walter E. Kreuzi, Cambridge: I am a member of the board of managers in the Cambridge Tuberculosis Association, and we used an old school building for a fresh-air school room. The association for the first year provided the extra clothing and the extra equipment. The

school board has since that time taken it up themselves, and gone right on.

I am very glad indeed to hear the result of Dr. Hawes' canvas of the opinions. I am especially delighted with the remarkable preponderance of opinion regarding the providing of children with food and air, and I am very pleased to note the considerable percentage of those favoring doing this at the expense of the city.

Dr. Arthur T. Cabot: In regard to the treatment of tuberculous children in our midst, there are two plans; one plan of having hospital schools such as at the Franklin Park School, and the other of treating these children in the hospital and turning them over to the hospital authorities.

I think that Miss Farmer, who comes in contact with the tuberculous children in her classes, is here and can give us some very valuable information.

Miss Gertrude L. Farmer, of the Social Service Department of the Massachusetts General Hospital: There is no special provision for the children that come to us at the hospital. Of course our problem is not only municipal but state also. About 50% of our cases come from all sorts of small communities and towns outside of Boston.

It has seemed to some of us that all children should have fresh air in the school and cleaner school rooms, and in addition to this all children should have open-air rooms.

I think that we need for children state sanatoria and day camps, so that the children who really have tuberculosis may be in a suitable place.

SYMPOSIUM ON OUT-DOOR SCHOOLS AND OPEN-AIR ROOMS FOR CHILDREN.

ARTICLE XXXIV.

OPEN-AIR ROOMS AND HOSPITAL SCHOOLS.

By GEORGE S. C. BADGER, M.D.
OF BOSTON,
AND JOHN B. HAWES, 2D, M.D.
OF BOSTON.

Report of Committee appointed at the Annual Meeting of the Associated Committees of The Massachusetts Medical Society for the Prevention and Control of Tuberculosis, June 13, 1911.

At the meeting of the Tuberculosis Section of the Massachusetts Medical Society, held at the Harvard Medical School on June 13th, 1911, there was held a symposium on the subject of Out-Door Schools and Open-Air Rooms for Children. At this meeting there were present many physicians from all parts of the state, school superintendents, and others from Massachusetts and elsewhere interested in the subject.

Papers were read by David Snedden, Esq., State Commissioner of Education, and by Dr. Thomas Leen of the Boston School Board. Shorter papers and remarks were presented by numerous other speakers whose opinions were of interest and value.

Prior to this meeting, at the suggestion of the Suffolk District sub-committee, of which Dr. Henry Jackson of Boston is chairman, a circular letter containing questions concerning the development and management of fresh-air rooms and out-door schools was sent out by the Tuberculosis Committees of the Massachusetts Medical Society to

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(*a*) members of the Associated Committees; (*b*) Massachusetts School Superintendents; (*c*) Boston School Physicians; and (*d*) certain other persons known to be interested. A preliminary report of the answers to these questions was presented at the meeting by the Secretary (see page 380). At the close of the meeting the Chairman, Dr. Arthur T. Cabot, appointed a committee of the undersigned to make a study of the papers and opinions presented at the meeting, and to prepare from this material a statement concerning fresh-air rooms and hospital schools, which it was hoped would prove of value to the various communities in this state and elsewhere who are contemplating the establishment of such rooms or schools. The report of the committee is herewith presented.

DEFINITION OF TERMS AND CLASSIFICATION OF CASES.

In the discussion of such a question as this it is of the utmost importance to have a clear idea of terms and definitions used, so as to avoid any possible confusion which might arise, and which indeed has already arisen. The terms "open-air schools," "fresh-air rooms," "open-air rooms," etc., should be applied to rooms in schools or in separate buildings near the schools, on the roofs, etc., where certain children are given instruction practically out of doors. These children are by no means actively tuberculous, although they may have tuberculosis in them, but they are anemic, run down, poorly developed and nourished, and unfit for work in the ordinary school room as at present ventilated. Such rooms will be referred to in this paper as "open-air rooms," and children attending them wlll be placed in Class A. The "open-air rooms" here referred to are similar to those now conducted in New York, Chicago, Rochester and elsewhere.

The "hospital school" is absolutely different. This name has been applied to an institution run under the auspices

of hospital authorities, and in connection with a hospital or sanatorium, although preferably not in immediate proximity to either hospital or sanatorium. In other words it is a day camp for tuberculous children. Such an institution is for those children manifestly tuberculous, who plainly ought not to go to our regular public schools or even open-air rooms, and yet who are not sick enough to require bed treatment in a hospital or sanatorium. Such children constitute what we call Class B.

Our problem then consists in properly caring for (1) children in Class A, anemic, run down, but not actively tuberculous, in open-air rooms, and (2) children in Class B, actively tuberculous, to a greater or less extent, in hospital schools.

THE NEED OF OPEN-AIR ROOMS AND THEIR PLACE IN THE EDUCATIONAL PROGRAM.

The need of open-air rooms in all school buildings is now recognized. Out of 200 answers to the letter of inquiry above referred to, 196 were in favor of such rooms and only four opposed. These figures express the general opinion everywhere. Boston already has four such rooms and plans to have forty. New York and Chicago are instituting them on a large scale. After all, as Mr. Snedden pointed out: "To allow children all the fresh air they need is but in accord with the general modern principle of getting back to nature. Schools, as such, are abnormal for young children, and the less artificial is the atmosphere the children breathe the better off they will be." In addition to the good effect of such rooms on those children actually in them, as pointed out by many of the speakers, the effect on the ventilation of other rooms, the tendency to open windows, and the effect of the rapid improvement and return to normal of the open-air room children on the other children and their teachers is very marked.

It is evident then that the general principle of open-air

rooms is recognized and established. It is coming to be recognized that all systems of artificial ventilation are practically failures, and provide a poor substitute in the shape of "baked air" for the genuine article. The time is coming, and is not far off, when all children will be in open-air rooms and all schools will be open-air schools. When this time comes it is to be hoped that the campaign will be carried into the children's homes, so that fresh air at night and proper hygiene at home will go hand in hand with proper conditions at the schools.

THE FEEDING OF CHILDREN ATTENDING OPEN-AIR ROOMS.

The question as to the feeding of children who attend open-air rooms has been much discussed. The great majority of physicians and educators now believe that children who need more fresh air also need more nourishment. Indeed one observer, Dr. Benjamin T. Loring, a Boston school physician, is of the opinion that the problem is more one of nutrition than of fresh air; that it is useless to increase the metabolism of our children and to give them a better appetite, unless they get more food of the right kind. He vividly describes a series of twelve children selected at random, investigated by his nurses, where the lack of proper food at home seemed to be the great factor in the child's poor condition. Dr. Thomas F. Leen, of the Boston School Board, expressed his opinion that investigations carried on by means of the Boston school nurses have led him to the conclusion that families where there is such poverty as to actually prevent children getting enough food are very few indeed. He believed it to be more a question of poorly trained parents, who gave their children improper food, tea, coffee, etc., and allowed them to spend their extra pennies for pickles, candy, and the like, than one of actual inability to supply the food itself. However this may be, it is the general opinion that children attend-

ing open-air rooms are better off with some extra food in the shape of lunches in the middle of the morning and afternoon sessions, the children being expected to return home for their mid-day meal.

The Advisory Committee on School Hygiene for the Boston Schools was asked by the School Committee for their opinion concerning the feeding of school children. The Committee decided that the pupils in the open-air rooms ought to have a lunch, and that this lunch, so far as possible, should be brought from the home, and a suitable place provided for the eating of it, that school lunches, to be paid for by the pupils, should be provided for those not bringing a lunch, and that the school feeding of the normally healthy child was not necessary.

THE COST OF PROVIDING LUNCHES FOR SCHOOL CHILDREN.

The cost of lunches for children, and who should pay for them, is another point concerning which there has been great difference of opinion. Mr. Frank Mann, Secretary of the Tuberculosis Committee of the New York Charity Organization Society, stated that in his city the cost of such lunches for children attending their open-air rooms was in the neighborhood of fifteen cents per child per day. This sum, however, includes not only lunches in the middle of the forenoon and afternoon, but a mid-day meal, which in Boston, and, we believe, in the majority of municipalities, is unnecessary. Mr. Mann stated that if the number of such open-air rooms was to be increased, as was already planned, the cost of these lunches would be prohibitive, and the idea of feeding school children would have to be given up. Dr. Thomas F. Harrington stated that in Boston lunches consisting of bread and butter and jam, have been served to school children, at a cost not exceeding two cents per child per day. This being the case, it seemed eminently practical for the child's parents to provide the

small sum necessary for these lunches. In those rare cases where even this small sum cannot be provided, only after most careful investigation by competent authorities, should lunches, in our opinion, be given by private charities.

It was the general concensus of opinion at this meeting that it was a dangerous precedent for school authorities to provide free lunches for children attending open-air rooms, but that lunches should be provided which could be secured at a nominal cost.

CLOTHING FOR CHILDREN ATTENDING OPEN-AIR ROOMS.

Children attending such open-air rooms in the winter time will need an extra amount of suitable clothing. This clothing, in most instances, has been in the shape of a canvas or blanket bag or wrap, extending high up, and fastening just under the arms. This, in addition to the child's own cap and mittens or gloves, when these are necessary, is all that is needed to keep the child warm and comfortable. It was the general expression of opinion that such wraps might well be provided by the school authorities and loaned to the school children, as they now do school books, slates and pencils.

CONSTRUCTION OF OPEN-AIR ROOMS.

The construction of such open-air rooms has shown wide variation. Mr. Riley, Superintendent of Schools of Holyoke, described a new school building, in which certain corner rooms south and east were provided with five large windows, eight feet high and three feet wide, pivoted top and bottom, so that they could be thrown wide open. Other rooms were described, in which the windows contained one large sash, hinged at the top, so that it could be swung in up to the ceiling.

In Boston, six rooms in four school buildings have been converted into open-air rooms. No special changes have been made in the rooms — the windows have been opened

top and bottom, and kept open. The children have worn their own clothing, hats, coats, gloves, and at times overshoes. On very cold days they sit part of the time on the desks, their feet in the chairs. In the warmer weather some of the exercises are conducted in the yard, where seats are provided. These children have a lunch served to them in the middle of the forenoon, for which they pay two cents.

To these open-air rooms are sent those children who are physically below par. Actively tuberculous children are excluded. Children returning to school after an illness are often sent here until able to enter their regular class.

The benefit derived from these open-air rooms is to be measured not so much by a gain in weight, or by the results of a medical examination, as by an increase in the efficiency of the work and regularity of attendance. The teachers report a decided increase, and they are enthusiastic in their advocacy of the plan. The School Committee has voted to open forty more such rooms for open-air classes.

Other such rooms have been arranged on the roofs of school buildings, or in shacks or pavilions in the school yards. It is not the purpose of this committee to more than mention these various forms of fresh-air rooms, as in each community the problem will be an individual one, to be dealt with as circumstances allow.

SELECTION OF CHILDREN FOR OPEN-AIR ROOMS.

The selection of children who should attend fresh-air rooms depends upon the facilities at hand. The answers to this question in our circular letter showed that a vast majority were in favor of this being done by the school authorities, school teachers, nurses, and physicians acting together. Several called attention to the fact that the school teacher really knew more about the condition of the children, as regards efficiency and health, than anyone else.

She would naturally be the one to point out such children to the school nurse or the school physician. The instances where experts of a special department would have to be called in must in all probability be very few. Dr. Leen, Dr. Badger and others urged the strengthening of the medical side of the school department as the best means of meeting this problem, and that with more and better school physicians, or perhaps fewer and better paid school physicians, and more school nurses, the problem of selecting candidates for open-air rooms would be a simple one.

THE CONTROL OF FRESH-AIR ROOMS.

The question as to who should control and administer such open-air rooms needs little comment. That the school board, with a strong and efficient medical department of physicians and nurses, suitably paid and properly maintained, should have complete supervision over such rooms, is acknowledged by all.

THE SIZE OF THE PROBLEM.

The number of children who would be suitable candidates for open-air room treatment have been variously estimated at from 5% to 18% of the total number. Careful investigators of later years have put this figure much lower than that of earlier enthusiastic but perhaps less accurate observers. Accurate statistics are surprisingly few. From a careful investigation of 5,000 children in Boston, and from similar work in England and elsewhere, it would seem that not more than 2% of the total number of children are anemic, ill nourished and run down, so as to be better off in open-air rooms.

In Boston, as already stated, they have four open-air rooms, and plan to have forty. In New York they have five, and are arranging for twenty more. Many others are already in existence elsewhere and others are being planned for all over the country.

HOSPITAL SCHOOLS.

As stated above in this report, the hospital school is really a day camp for tuberculous children who are too sick to attend the regular public schools or even fresh-air school rooms in such a school, and yet are not sick enough to require bed treatment in a hospital for consumptives. Such a day camp is in no way a part of the school system; the really tuberculous child should not go to a school of any kind, but must leave it and enter upon treatment for her disease. The Franklin Park School, or, more properly, Day Camp, was an example of this type.

To such an institution as this children came from their homes from all over the city and spent the day in the fresh air. In addition to this they received morning and afternoon lunches, a good midday meal, and such schooling necessary to occupy their minds, as their condition warranted. The home conditions were investigated by the nurses in charge, and were improved to the greatest possible extent. Many children attended this hospital school with great benefit, whose parents would not think of allowing them to attend an institution similar in all other respects but situated on the grounds of a large consumptives' hospital.

Under ideal conditions children attending such a day camp as they improved would first return to the open-air room in the public school to which they naturally belonged, or if they grew worse and needed bed treatment they would be sent as patients to the proper tuberculosis hospital.

The educational effect upon the parents and the community at large of children attending such a school is acknowledged to be very great. It is only in comparatively large communities, however, that such a hospital school as this would be necessary or feasible, as the percentage of children who belong to this class which would make them suitable candidates for the hospital school is bound to be very small indeed.

The chief criticism against the Franklin Park School was that the distance that many of the children had to travel to and from their homes each day was too great; in a large city such as Boston there should be several such camps scattered over the city, at which tuberculous children could be properly cared for at a comparatively short distance from their homes.

SUMMARY.

In conclusion, we would call attention to the fact that it is not fresh-air rooms or hospital schools alone which are to solve this problem. These are but two factors in the far wider subject of school hygiene. It is by strengthening, broadening and combining all forces that the condition of the child is to be improved. Children must be properly taught the principles of hygiene and right living; fresh air must be provided for all school children; trained school physicians must be properly paid for the work they do; school nurses must everywhere be made a part of the system as the most important means of making the school physicians' work effective; there must be fresh-air rooms for ailing children; proper hospital facilities for acutely sick children, and tuberculosis camps or wards for consumptive children. Above all, the public must be educated to see the reason for all this, and to insist upon it.

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WALTER L. BURRAGE, M.D.,
Secretary.

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